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ANNALS

OF

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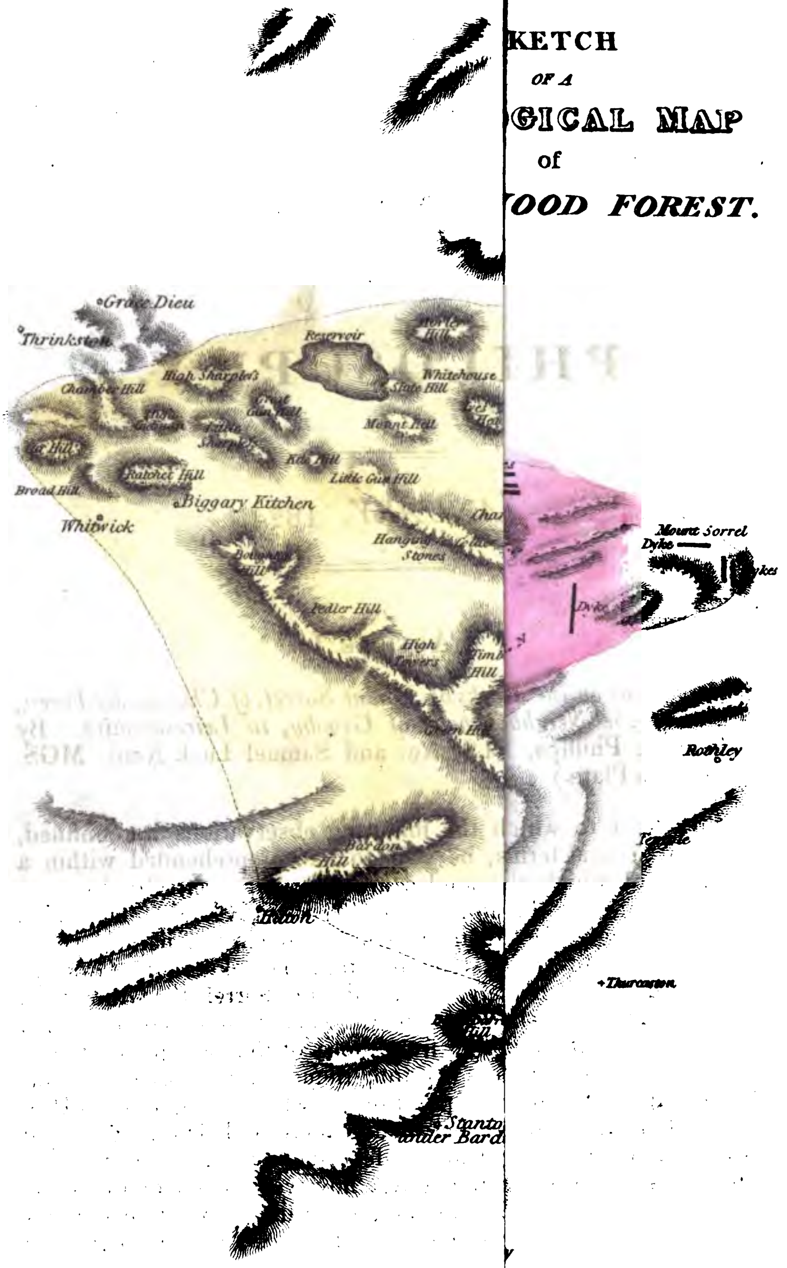
ERRATA.

Page 29, line 18, *for* Lapulin, *read* Lupulin.
83, line 10, *for* Ther. *read* Thus.
325, line 19, *for* infringe, *read* impinge.
327, line 2, *for* assent, *read* agent.
384, line 4 from bottom, *for* L. Italioa, *read* L. Italica.
388, line 4, *for* Harleston, *read* Starston.
5, *for* Clarton, *read* Clacton.

KETCH
OF A
GICAL MAP
of
OOD FOREST.



SCALE. ONE INCH TO A MILE.

ANNALS

OF

PHILOSOPHY.

JANUARY, 1824.

ARTICLE I.

Observations on the Rocks of Mount Sorrel, of Charnwood Forest, and of the Neighbourhood of Grooby, in Leicestershire. By William Phillips, FLS. &c. and Samuel Luck Kent, MGS. (With a Plate.)

The tract to which the following observations are confined, may, in general terms, be said to be comprehended within a triangle, of which the angles are Mount Sorrel, Grooby, and Thrinkston. (See the annexed Map[*], Plate XXIV.) Two sides of this triangle are about nine miles in length, namely, from Mount Sorrel to Thrinkston, and from Thrinkston to Grooby; the third side, namely, from Mount Sorrel to Grooby, is between five and six miles long. This triangle comprehends rocks remarkably differing from those of the vast plain of new red sandstone which they overtop, though not to any considerable elevation, the highest point of the whole being Bardon Hill, situated nearly midway between Grooby and Thrinkston, and which attains the height of 853 feet above the level of the sea. On the south-west of this tract, however, rocks of the same nature as those near Grooby, are, according to Mr. Greenough's map, to be found for some little distance; but the extent of these we did not visit.

The rocks of the area we have mentioned vary greatly in their external characters; but before we proceed to describe them, it may not be amiss to give a general outline of the principal feature of the district in question.

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[*] The accompanying map is not given as an accurate representation of the forms of the hills, but chiefly to assist the reader, or the traveller, in forming some idea of their relative position.

From the height which has already been mentioned, it will at once be decided that it cannot be considered as mountainous, but only hilly. The whole tract, however, may be divided into three parts, when viewed in relation to its surface, and its geological features. As regards the latter, it is divided only into two parts in Mr. Greenough's map; the green colour correctly denoting the existence of trap rocks on the south-eastern parts of the district, being incorrectly carried up to the north-eastern, where an essentially different rock prevails, to the exclusion of all others, and which is coloured red in the annexed map. These two extremes are less elevated than the central and western parts, which consist of another rock perfectly dissimilar to either of the former.

The extreme extent of this tract on the east, is formed by the cliffs above the town of Mount Sorrel: from near the summit of these cliffs, which may be assumed scarcely to exceed at their highest part the height of 150 feet above the river Soar, the country descends gently on the west and south-west for about two miles, if we except two or three well wooded hills, termed Buddon's Wood, and attains its greatest depression along a line extending by Swithland, Rushfield, and Woodhouse, to Loughborough Park; and here the country is at least as low as the general level of the red sandstone surrounding the tract of which we are treating. The small patch forming the south-east angle of our tract, which is coloured green in the annexed map, on the north and north-west of Grooby, is generally of inconsiderable height, the highest point being the knowl on which the windmill stands close to Markfield. The remainder of our district (coloured yellow in the map) may be considered as one large hill, rising into frequent eminences, of which one of the most lofty near the centre, Beacon Hill, is but little lower than Bardon Hill, the highest point of the whole. The short and numerous valleys dividing these eminences, though much above the general level of the new red sandstone, are nevertheless covered by it in several instances; and it is manifest that its beds repose on the western side of Beacon Hill.

The numerous eminences already adverted to have received each its own designation as a separate hill, and it is chiefly on the summits of these that the nature of the rocks constituting them is to be perceived, being frequently crowned by rugged and bare masses, which, particularly as viewed from near Grace Dieu, have a serrated outline. This district for some miles east of Thrinkston, where the hills are numerous and very rugged, is little or not at all cultivated, the depressions between and among them being covered by a long and very coarse grass, beneath which, in some instances, as near Pedler Hill, the ground is extremely soft, and even swampy. The other parts of the district, however, differ greatly from this in their general aspect, being often highly cultivated, even to near the summits of the hills, as in the instance of the south-western side of Beacon Hill, while the lower parts, in some few instances, are well wooded, and even some of the loftier summits are crowned with woods, as is the case with those termed the Outwoods, NW of Beacon Hill, and the hill in which are situated those slate quarries near Swithland, which are the most distant from that place on the SE, and Bardon Hill to its very summit. Everywhere, however, except where the eminences are crowned by bare rocks, a herbage forms the surface, covering a more or less deep alluvium of fawn-coloured, or reddish and loose earth, as was frequently manifested by the labours of the mole, even on some of the most elevated ridges; and that this alluvium is at least occasionally of considerable depth, is proved on the side of Whittle Hill, in which are situated many little quarries of five to fifteen feet deep, wrought in search of fragments and loose pieces of that peculiar variety of the rock of the forest, so greatly used in different parts of the kingdom for setting penknives, and which is termed the Charnwood or Charley Forest hone.

This very slight sketch of the external characters of this tract, will evince that the opportunities of judging of the nature of its rocks is far more limited than could be wished, but suffices at least to furnish such information as may serve, if not to determine, at least to afford some probable notion of the relative æras o which they belong, even though some points must necessarily be left undecided.

These difficulties are, first, that having found it impossible to discover the actual connexion of any two of the three rocks constituting this tract, and which differ greatly in aspect and composition, we are deprived of any direct means of ascertaining their relative periods of formation; and, secondly, that neither the one nor the other is seen reposing upon any other rock which, in that case, might be assumed to be anterior, and might therefore serve, in some degree, perhaps, to assist in deciding their relative age. It is indeed true that we are justified in considering them older than the surrounding new red sandstone, since its beds actually repose upon these rocks, which pass away gradually beneath them. This circumstance, which is visible in a quarry at the eastern end of the cliffs above the town of Mount Sorrel, and to a still greater extent at both extremes of an old and deserted slate quarry near Swithland, would seem to prove, that in the section annexed by the Rev. W. D. Conybeare to the "Outlines of the Geology of England and Wales," the rocks of Charnwood would have been represented with somewhat greater accuracy, if instead of delineating the beds of the new red sandstone as abutting against them, their extremes had been shown reposing on them, and the rocks of this district passing gradually, but at a considerable angle, beneath them. As represented in that section, the rocks of the Forest may possibly serve to convey a notion that they have been thrust up by some subterranean force—a notion which we conceive would be erroneous, arguing from the remarkable regularity with which the sandstone beds repose on the rocks, dipping wherever they are visible at an angle not less than six nor greater than eight degrees.

The regularity of the sandstone beds seems also to come in proof of another fact; namely, that the rocks of the Forest have not suffered by convulsion—a conclusion strengthened by the observation that the direction of the slaty cleavage of these rocks, which is mostly apparent, or becomes so by the assistance of the hammer, is everywhere either NW by W, and SE by E, or differs but very slightly from it towards the W and E.

For the reasons which have already been given, we shall treat separately of the rocks of the three parts into which this district is divided, as the Mount Sorrel, the Charnwood, and the Groby tracts; and first of the former.

Of the Rocks of Mount Sorrel.

The rocks of Mount Sorrel are admirably laid open to view by means of a line of quarries overhanging the town, and giving to these rocks the appearance of cliffs, perhaps one-third of a mile in length, and of the average height of nearly 100 feet, but not absolutely continuous. Other small and detached quarries are wrought on the eastern side, the whole being chiefly for the purposes of road-making. The rock when sound is broken into the proper form for paving stones, which are shipped on the Soar for various parts of the kingdom, when less so, for mending roads in lieu of the gravel employed in the neighbourhood of London, for which purpose the Mount Sorrel rock is far superior.* Immediately on quitting the town on the W, the rocks sink beneath a comparatively low and verdant covering, for some little distance, and then again swell into trifling elevations a little south of the road passing from Mount Sorrel to Quornden, or Quorn, as it is commonly termed by the inhabitants of both places. In Buddon's Wood, which occupies the greater part of these little eminences, and within half a mile of Quornden, are situated two or three inconsiderable quarries, one of which, however, is remarkable, as will presently be noted, for the veins or dykes traversing the rock.

The openings abovementioned, together with a small one situated about a mile nearly SW of Mount Sorrel, and called Simpson's Pit, remarkable also for its exhibiting the appearance of a very determinate dyke, form the whole catalogue of the quarries observable in this rock, and which offer the principal opportunity for studying its nature. Some rocks, however, appear *in situ* on an eminence between the town and Buddon's Wood; while some also overtop the surface of Rothley Plain, of which the gradual descent begins at the abovementioned quarry, called Simpson's Pit; and in the earlier part of the descent of this plain, rocks perfectly resembling those of Mount Sorrel occasionally overtop the general surface, sinking, as has already been observed, ultimately beneath it, and so completely that it is impossible to discover their connexion with those of the Forest.

---

* We request thus to express our obligations to —— Jackson, Esq. residing at Mount Sorrel, and the present proprietor of the quarries, for his polite attention in directing our observation to every point which he considered the most likely to interest us.

The aspect of the Mount Sorrel rock is granitic, and hand specimens may, perhaps, be found, of which the ingredients appear to be confined to those commonly considered as being essential to granite; namely, quartz, felspar, and mica; for the hornblende which generally is sufficiently apparent, and which often abounds, is occasionally so nearly wanting, or so minute in small specimens, that it may easily be overlooked. In reality, however, much of that which appears to be felspar, is not that mineral, but cleavelandite. Epidote occasionally enters into its composition, but is more generally found in small nests or veins, with semi-transparent quartz, when it is sometimes associated with magnesian carbonate of lime, which cleaves into rhomboids, and slowly effervesces, in diluted muriatic acid; silvery talc appears sometimes on the quartz found in veins or nests in the rock, and the same substance of various colours enters into the composition of some of those which overtop the grassy slope of Rothley Plain. Chlorite also in small quantity is sometimes diffused through the mass, and occasionally appears traversing it in thin irregular veins.

The quartz entering into the composition of this rock is transparent or semitransparent, the mica in thin hexagonal plates, and the surfaces produced by dividing them parallel to the terminal plane are very splendent and of a colour nearly approaching to black, but by transmitted light the laminæ appear of a dingy-brown. The hornblende is of a dark bottle-green, approaching to black. The felspar and cleavelandite, which are almost constantly the prevailing substances, vary greatly in colour, are intermingled in the mass, and cannot always be distinguished from each other by their external characters. Both are commonly red or reddish, and this colour is sometimes so powerful in the cleavelandite as to impart to it the aspect of red jasper, particularly whenever it assumes in any degree the appearance of a vein; the planes produced by fracture are then generally curvilinear, and without lustre. Sometimes, however, the felspar and cleavelandite are intermixed, either simply, or in such a manner as to impart to the rock a porphyritic character; the felspar is generally translucent, and the imbedded cleavelandite white and nearly opaque, and the other ingredients of the rock then form a small grained paste. We were not in the first instance aware of the intermixture of these two minerals in this rock, and considering the whole as felspar, we should have contented ourselves with the observation that a part of it yields to the pressure of the edge of the hard mineralogical knife, which felspar does not, but for the discovery of M. Lévy, that much which has been considered as felspar is cleavelandite; the announcement of this in the *Annals* for November last, induced us to examine all the varieties of this rock with great attention, and we have been convinced of the intermixture of the two minerals by procuring fragments from the same specimen; which, submitted to the reflective goniometer, afforded us separately the angles of the two substances; felspar cleaves with ease only in that direction which affords an angle of 90°; while cleavelandite yields with nearly equal ease parallel to all the planes of its primary crystal, and we have consequently obtained the measurement of all its angles.

The Mount Sorrel rock thus constituted* will be judged by some to be a syenite; while others will consider it to be a granite of the compound kind described by Dr. Mac Culloch, in his excellent Treatise on Rocks, under the Third Division, relating to that rock (p. 238), and it is to be regretted that no sufficient means of determining its actual geological position (which alone might settle the question) is afforded, since it is not seen in connexion with any other rock, save the beds of the new red sandstone, which, as has already been stated, repose on it.

But there are still some circumstances regarding this rock which merit attention. It not unfrequently includes either irregular or somewhat spherical masses, varying in size from about an inch in diameter to nearly a foot, and possessing a totally different aspect to the rock itself; some of these have at first sight the appearance of fragments, but not even a close inspection with a glass can discover the precise line of junction with the rock. Their aspect is fine-grained, and their composition appears to be, minute quartz, felspar, hornblende, and chlorite, apparently imbedded in a substance of a hair-brown colour, which yields to the knife readily, and resembles steatite. The mass partakes of a brown colour with a tinge of green, especially when moistened: in one instance, specks of yellow copper ore and whitish pyrites were apparent; in another, the same ingredients are visible, and as effervescence is produced by the application of diluted muriatic acid, it may be assumed also to include calcareous spar.

This rock also contains veins or dykes: the substances of these differ greatly in aspect and composition. In some instances they are scarcely discernible from the rock itself which incloses them; in others, they appear to consist chiefly of steatite; and in others, of a substance which occasionally has much resemblance to the inclosed masses just adverted to, and which, in one instance at least, is not to be distinguished by the eye from fine-grained basalt.

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* We must not make any particular exception of this rock, on account of its containing cleavelandite abundantly. This mineral has since been found entering into the composition of a porphyritic granite from Cornwall; in the granite of Shap in Westmoreland, and in a porphyry from Glen Tilt in Scotland, as announced in the *Annals* for December; and still more recently in the porphyritic rocks of Charnwood Forest; and probably also in the neighbouring trap rocks of Grooby, in Leicestershire; in each of these it is accompanied by felspar.

The dykes of which the substance greatly resembles the rock of Mount Sorrel, though in a somewhat disintegrated state, and containing some reddish steatite, are situated not far from the eastern extremity of the line of quarries at the back of the town: they are two in number, both run E and W, have pretty determinate walls, and are visible for about 12 feet; that is, to the depth at which the rock has been quarried where they occur. One is about 20 inches wide at top, and after narrowing a little, divides into two narrow branches; the other is about two feet wide, and underlies a little towards the north.

The veins or dykes which consist primarily of steatite, traverse a rock perfectly resembling the reddest variety of Mount Sorrel, in Beaumont's quarry, which is situated on the edge of Buddon's Wood, beside a road leading from Quornden to Rothley, and about half a mile from the former place. Three veins are here visible which are parallel, run nearly due E and W, and each may be traced on an average about 30 feet, the walls being mostly very determinate: the two southern veins are about four feet apart, and the third is about 22 feet on the north of the nearest to it: they vary from 18 inches to two feet in thickness. The substance of the southernmost vein, which underlies a little to the south, is a greenish steatite, which is translucent, yields to the pressure of the nail, and incloses a few specks of hornblende and talc. Externally it is, however, in a state of disintegration probably from exposure, and readily crumbles down into a greyish-white, and somewhat unctuous powder; and in places it is associated with a harder substance which is granular, and which has the aspect of steatite of a mixed red and green colour, and incloses quartz and hornblende. The steatite of the middle and northernmost veins, which are nearly or quite vertical, is not quite so soft; it incloses specks of hornblende and chlorite, together with small masses composed of a reddish substance resembling hornstone, or compact felspar, associated with decomposing hornblende. It is observed by Dr. Mac Culloch, in treating of some of the more compound varieties of granite described by him, that it is doubtful whether the apparent steatite of some varieties may not be decomposed talc or chlorite, an observation that may, perhaps, have some bearing on the steatite of these veins.

On the face of the rock S of these veins, we observed a mass of a bluish cast, about 20 feet wide, and 6 feet high, greatly resembling the substance of the dykes, presently to be noticed, and reposing on the rock of the quarry, and bounded by it on each side; we could not, however, perceive the actual junction.

There are two dykes of which the substances much resemble each other, except that in one part of one of them the rock has greatly the external characters and appearance of a fine-grained basalt. Just beside the road that divides the line of quarries which overhang Mount Sorrel, we perceived among other stones broken up for the repair of the roads, many fragments of a bluish hue; and on inquiries respecting the place from which they were taken, a vein running nearly N and S, and about a foot in thickness, was described to us as existing beneath the rubbish just where the road turns to the left round the house inhabited by a clergyman whose name we did not learn, but on the opposite side of the road. We, therefore, did not see the dyke in question. The rock has, in no inconsiderable degree, the aspect of a granular basalt, and though considerably hard, it yields to pressure with the knife a grey powder pretty readily. The only discernible substances in it are extremely minute and slender crystals of transparent felspar, and sometimes cubic, sometimes octohedral crystals of magnetic iron pyrites, together with occasional specks of a yellowish-white substance which appears to be laminated, but does not effervesce on the application of diluted muriatic acid. The base or imbedding substance is not sufficiently characterized to enable us to decide upon its nature; it is, however, considerably soft, and when reduced to thin fragments is translucent, of a slightly brownish hue, and contains extremely minute specks of a dark-green colour. It seems, however, intimately connected with that of the mass constituting a part of the dyke about to be described. This dyke is situated in the small quarry called Simpson's Pit, in a rock perfectly resembling that of Mount Sorrel, and about a mile SW of the town, at the head of Rothley Plain. It appears to run through the mound in which the quarry is situated nearly due N and S, is visible for about 20 feet in height, is six feet wide at the lowest visible place, and somewhat narrower at the surface, is nearly perpendicular to the horizon, has very determinate walls, and reappears about one-fourth of a mile SW in a field occupied by the landlord of the Crown Inn at Mount Sorrel. The substance of this dyke as it appears in the quarry, is of a bluish cast, yields readily to the knife a grey powder, but is translucent in thin pieces, exhibiting, by the assistance of the glass, a multitude of dark specks which appear occasionally to tinge the mass of a green colour; it scratches glass feebly (a circumstance not anticipated from the use of the knife, and which may be owing to the presence here and there of minute and slender crystals of felspar, or of quartz too finely intermixed for observation), but its substance is left on the glass. The mass itself has somewhat the aspect of steatite, an aspect which is not to be observed where the dyke reappears below; here it very greatly resembles a granular basalt, and is sufficiently hard to admit of a flat conchoidal fracture; it yields to the knife, though not without considerable pressure; is of a dark colour, owing to the thorough intermixture of specks of a dark-green substance, and the slender crystals of felspar are innumerable; but neither quartz nor any other mineral is to be detected even by the help of a highly magnifying glass. The rock in immediate contact with this dyke did not appear to have suffered any alteration.

The rocks of Mount Sorrel are occasionally also traversed by small veins which more decidedly belong to the rock itself. These veins sometimes resemble granular felspar, sometimes granular quartz: they are always of a red hue: when of felspar, they appear to include minute specks of quartz, hornblende, and chlorite; while those of quartz inclose crystals of quartz, felspar, hornblende, and iron pyrites. The veins of white or translucent quartz usually contain chlorite, and sometimes epidote.

No appearances of regular stratification are to be observed in any of the quarries of this rock; at the western termination of their line at the back of the town, however, slabs of considerable dimension are procured, owing to the presence of nearly parallel fissures in two opposite directions, both being nearly vertical, and those producing the largest plane, running nearly due E and W. In the quarry at the other side of the hill, and due E of the windmill on its summit, are to be seen some natural cleavages in various directions, which, without sufficient caution, might be mistaken for dykes.

Of the Rocks of Charnwood Forest.

The rocks of the district of Charnwood Forest, coloured yellow in the accompanying sketch, differ greatly in respect of aspect and composition, and vary so greatly in their external characters, that although, from the circumstances attending them, it is impossible to doubt their common origin, there are varieties which, if taken separately, would, without previous acquaintance with them, be judged to be of very different ages, since even some of the proximate rocks appear to possess as little mineralogical affinity as chalk and flint.

The greater part of these rocks are certainly schistose, but in degrees varying from a fissility approaching that of common slate to that of the rudest kind in which this seeming consequence of structure is to be perceived; even the most completely schistose varieties differ greatly in composition and external character; while those that are least so often contain fragments of other substances which, perhaps, tend to characterise the whole (since all the varieties occur interstratified without any order), as belonging to that class of rocks which are denominated by Dr. Mac Culloch primitive slates; by others greywacke. And here it may be pertinent to remark, that in no instance was any trace of organic exuviæ observed.

same direction and dip as the more perfect slates of the quarries; but although they occur in layers varying from one-eighth of an inch to two inches in thickness, these layers have not the same direction as that of the cleavage; but this we shall presently describe.

The preceding may be said to constitute the principal varieties of the imbedding substance or paste in what may be termed its purer state; in which, however, it is sometimes rendered irregularly slaty, by the intervention of layers of chlorite; the substance then, as well as in some of the purer varieties, somewhat resembles potstone.

The quartz which forms so important a feature of the slates, and which doubtless is more finely disseminated throughout the harder varieties of the rock in the state of a mechanical mixture, occurs in two instances at least under very different circumstances; in one as a bed or vein of considerable dimension and extent, of granular quartz, having the aspect of quartz rock, and consisting apparently of grains of pure and nearly transparent quartz closely and strongly aggregated without any intervening cement; this bed or vein occurs in Grooby Park, and appeared to us to run in the direction of the slaty cleavage observable in the quarries and elsewhere, and with the same dip. Slates were observable on each side, though not in close contact with it. In the other instance, compact quartz of a green colour, and which but for the translucency of the edges might be mistaken for green jasper, exists in layers in a large mass in a field behind the last house of Newton Linford, which is most distant from Grooby. It lies immediately between layers of the granular looking varieties of the coarse slates before described, and without any precise line of demarcation. But this rock will be more fully described in adverting to the subject of the stratification of the rocks of this district.

Hitherto we have not mentioned these rocks in their more ordinary state of porphyries; the aspect of these differs greatly in different places; but whatsoever their nature may be, they are usually found connected with those we have already described, and generally in layers alternating with them without any regular order of succession. Sometimes, however, they appear to form almost exclusively the rocky eminences, already described as receiving each its own individual name, and especially those of the western part of the forest, near to Thrinkston.

The imbedded substances of the porphyritic rocks are various; these consist of transparent crystals of quartz, which generally are small, or of translucent crystals of felspar, often very minute, and which frequently are macles, or hemitrope crystals, and also of cleavelandite; these substances occur either separately (Broad Hill, Bardon Hill, &c.) or together in the same mass (High Swanmore, High Sharpless, &c.) and they are rarely abundant. Not unfrequently, however, instead of regular the various forms in which these substances appear. In some instances, the chlorite, instead of being disseminated throughout the mass, is arranged in irregular layers, thus imparting a schistose character to it (Windgate Hill, Great Bucks Hill, &c.)

Near Swithland are several quarries of the slate just described, one of which is estimated at 150 feet in length, and as much in breadth and depth. In all the quarries the run of the edge of the slaty cleavage is, as before related, NW by W, and SE by E, the broad surface of the slate dipping about 72° towards the NE, and it is to be remarked, that in each quarry, and particularly in the largest, there is a run of the best slate, about 15 feet wide, forming a kind of vein parallel with the direction of the cleavage, and flanked on each side by an inferior kind of slate; parallel also to this vein of superior slates, is another vein (if so it may be called) of a substance which is of a grey or greenish-grey colour, of a granular appearance, and consisting apparently of the imbedding substance or paste of the rocks of this district, in which chlorite is abundantly disseminated in particles and nests, and occasional specks of talc; quartz, from its hardness, may be concluded to be an ingredient of the mass, but is too small to be detected by the glass, and there is no appearance of texture.

Other instances in which this substance assumes a granular appearance occur, and without any perceptible mineral imbedded in it (Hangingstones, Beacon Hill, &c.): sometimes it forms a paste of a dull-green colour, which appears nearly or altogether homogeneous, possessing a splintery fracture, and varying greatly in hardness (above Whittle Hill quarry, High Swanmore, Short Cliff Hill); sometimes it is so soft as to yield readily to the knife; and the quartz is so sparingly disseminated through it, that although it slightly scratches window glass, the substance itself is left upon it as a grey powder (Hone-stone of Whittle Hill); in other instances, it is much harder, owing most probably to the presence of a greater proportion of quartz, but still so minute as to escape the eye (Bird Hill, Beacon Hill, and other places); these varieties are generally fissile; as well as others in which chlorite is perceptible as giving the greenish tint to the slate; in others, the green colour is totally wanting; the substance has then a yellowish tinge with every appearance of homogeneity, is frequently hard, sometimes so hard, owing to the abundant dissemination of siliceous particles throughout the mass, as to resist the knife, and it then assumes the aspect of compact felspar, or of hornstone (Bird Hill, base of Windgate Hill, &c.); but a substance of the same nature and degree of hardness also occurs, which is considerably schistose, greatly resembles flinty slate, is greenish, arising, as is visible by transmitted light through thin fragments, from the presence of chlorite often disposed in irregular lines (Morley Hill quarry). All these varieties are more or less slaty, and the laminæ have the same direction and dip as the more perfect slates of the quarries; but although they occur in layers varying from one-eighth of an inch to two inches in thickness, these layers have not the same direction as that of the cleavage; but this we shall presently describe.

The preceding may be said to constitute the principal varieties of the imbedding substance or paste in what may be termed its purer state; in which, however, it is sometimes rendered irregularly slaty, by the intervention of layers of chlorite; the substance then, as well as in some of the purer varieties, somewhat resembles potstone.

The quartz which forms so important a feature of the slates, and which doubtless is more finely disseminated throughout the harder varieties of the rock in the state of a mechanical mixture, occurs in two instances at least under very different circumstances; in one as a bed or vein of considerable dimension and extent, of granular quartz, having the aspect of quartz rock, and consisting apparently of grains of pure and nearly transparent quartz closely and strongly aggregated without any intervening cement; this bed or vein occurs in Grooby Park, and appeared to us to run in the direction of the slaty cleavage observable in the quarries and elsewhere, and with the same dip. Slates were observable on each side, though not in close contact with it. In the other instance, compact quartz of a green colour, and which but for the translucency of the edges might be mistaken for green jasper, exists in layers in a large mass in a field behind the last house of Newton Linford, which is most distant from Grooby. It lies immediately between layers of the granular looking varieties of the coarse slates before described, and without any precise line of demarcation. But this rock will be more fully described in adverting to the subject of the stratification of the rocks of this district.

Hitherto we have not mentioned these rocks in their more ordinary state of porphyries; the aspect of these differs greatly in different places; but whatsoever their nature may be, they are usually found connected with those we have already described, and generally in layers alternating with them without any regular order of succession. Sometimes, however, they appear to form almost exclusively the rocky eminences, already described as receiving each its own individual name, and especially those of the western part of the forest, near to Thrinkston.

The imbedded substances of the porphyritic rocks are various; these consist of transparent crystals of quartz, which generally are small, or of translucent crystals of felspar, often very minute, and which frequently are macles, or hemitrope crystals, and also of cleavelandite; these substances occur either separately (Broad Hill, Bardon Hill, &c.) or together in the same mass (High Swanmore, High Sharpless, &c.) and they are rarely abundant. Not unfrequently, however, instead of regular crystals, we find small angular fragments of quartz (base of High Swanmore, &c.) which, when the surface of the imbedding substance is somewhat decomposed into a white or greyish-white and granular mass, of which the particles possess but little cohesion, are left protruding. In other instances the fragments of quartz are intermixed with the regular crystals of quartz and felspar (High Swanmore, &c.). But the imbedded fragments sometimes possess all the characters of the slate already described as including irregular lines of chlorite, and as forming layers between the other varieties of the rock (Morley Hill quarry), or they resemble a reddish jasper which mostly is very hard, sometimes brittle (ridge of Bardon Hill), and occasionally other angular fragments appear of less decided character. It is, however, manifest to us, that the imbedded substance occasionally differs greatly from any of the preceding, in possessing altogether the appearance of some of the more homogeneous and greyish varieties of the rock itself, but much harder, and inclosing transparent crystals of quartz, and occasionally of felspar; these occur in nodules, varying from the size of a nut to several inches in diameter (Chamber Hill, Pedler Hill, ridge of Bardon Hill); and though they may be separated from the rock from which they project owing to the progress of its decomposition, yet there does not appear to be any precise line of separation between them; these, therefore, we consider to be of contemporaneous formation with the rock itself, as also may those numerous specks and patches be, which at first sight appear like imbedded fragments, but in reality present no line of separation from the rock itself, and are generally harder; they are of various colours, grey, brown, or red (SW foot of Beacon Hill, High Sharpless, Little Gun Hill, &c.)

The paste of these porphyries consists of all the varieties of the slaty rocks already described; but occasionally is much harder than any of them, since it resists the knife completely, owing, as we assume, to the mechanical diffusion of silex abundantly throughout the mass, which then partakes little or not at all of the schistose character (base of Windgate Hill, Pedler Hill, &c.) and resembles hornstone or compact felspar.

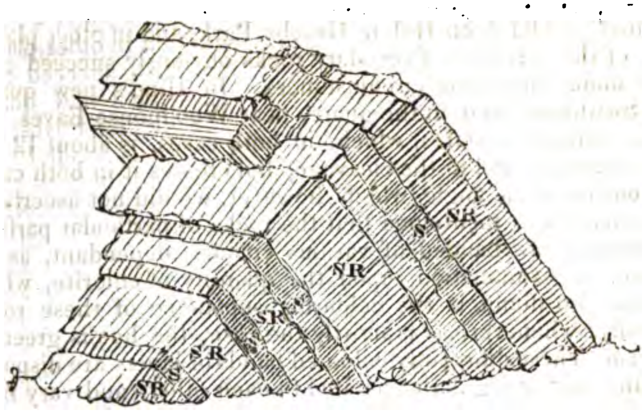
In regard to the actual stratification of the rocks of Charnwood Forest, two opposite appearances claim attention.

It has been mentioned that the direction of the cleavage of the slates and slaty rocks is everywhere nearly the same; that the edges of the slates uniformly run within a point or two of NW by W, and SE by E; and that the only difference is apparent in their more nearly approaching NW and SE. It has also been mentioned, that in the largest quarry near Swithland, a seam of the best slates runs in this direction, and parallel with it a seam or vein of rock, quite different from the slate; that in the quarry close to Woodhouse Eaves, a seam or vein of talc inclosing grains of quartz, takes the same course, and we may add that on Old John Hill, in Grooby Park, and in other places, some of the varieties of the slaty rocks obviously succeed each other under the same circumstances. In Hind's new quarry near Swithland, and in the quarry near Woodhouse Eaves, the course of the best slates, which, in both cases, is about 12 feet wide, is not parallel to the cleavage, but crosses it in both cases at about the same angle, which, however, we did not ascertain.

We have also mentioned that the rocks of particular parts of this district are banded of various colours, dependant, as we assume, on examination, upon the quantity of chlorite, which we have no doubt is the colouring substance of these rocks generally, whenever, as is mostly the case, the hue is green or greenish. These differently coloured bands or layers are disposed with the utmost regularity parallel to each other, and vary from the 16th of an inch, or even less, to some inches in thickness, and the slaty rocks thus disposed comprehend almost every variety found in the district. This appearance is particularly obvious on Morley Hill, in the northern part of the district, on the hill called Hanging Rocks on the eastern, on Old John Hill, on the south-eastern; while on the western side, as for some distance on the north of Whitwick, the rocks possess this character in a less degree, and there is more confusion apparent among them from the operation of external causes having left large blocks piled in the rudest manner on each other.

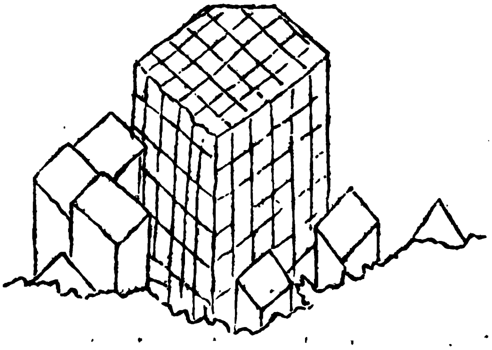
Although these bands or layers are visible in the rocks of several other hills besides those just mentioned, we select them because, though there is a perfect uniformity on each hill, in the dip and direction of the bands, each differs from the other in these respects. On Morley Hill, the dip is 45° to the N.; on the Hanging Rocks it is 45° to the E, and on Old John Hill 32° to the S, but that of the rocks at the foot of it are 45°. It is worthy of note, therefore, that as these several hills are situated on the extremes or outer edges of the tract, the bands of each dip away from the centre; and if these regular layers be taken as the order of deposit, and, therefore, as regular stratification, they tend to show it to have taken place in the mantle-shaped form.

But in whatever direction these layers may dip, it is remarkable that the dip and direction of the more slaty parts of the rock is invariably as above quoted, and it is impossible to exhibit a more striking instance of this fact, than is manifest in a rock situated in a field at the south-western foot of Old John Hill, and immediately behind the last house in Newton Linford.



This rock, represented by the above sketch, is about 12 feet high, and is constituted of alternate layers of green or greenish rocks more or less slaty (SR), and of blue slates (S); the central band consists, as do certain of the others, of some of the more granular looking varieties of rock, alternating with that which has already been described as greatly resembling a green jasper; there is no precise line of demarcation between them, nor does there appear to be any schistose structure parallel with these bands, except what is produced by long exposure to the atmosphere, which acts of course differently on substances apparently composed of very different materials, or, more accurately speaking perhaps, of the same materials in very different proportion, and consequent state of aggregation. The bands here, as near the summit of the hill, dip to the S, but the angle differs; above it is 32°, here it is 45°; the cleavage of the slates, however, which are interstratified with the rock, is in another direction, being perfectly consistent with that of the actual slates in all other parts of the tract; namely, 72° to the NE; and it is remarkable that the weathering of the rocks lying between the slates has a tendency to separate them chiefly in the same direction.

The weathering of these rocks, however, when composed of more uniform materials, is too striking in one or two cases to be passed over in silence. The masses chiefly seen on the hill termed the Hanging Rocks, have almost altogether the form of tetrahedrons, from two to six or eight feet high resting on the base, and having one plane constantly dipping towards the E at an angle of 45°, and this plane is parallel to the variously coloured bands of which the whole mass is constituted, and which in some cases have separated by the action of the atmosphere upon them. The other instance is on the summit of Beacon Hill: here the action has been carried still further: a single rock about eight feet high stands in the form of a doubly oblique prism, of which only the base is beneath the surface.



This prism is traversed by crevices, or by indications of them marked by imperfect lines parallel to all its planes; and so complete has been the action of the elements in some parts of this rock, that we succeeded in extricating from it some natural fragments in the form of the rock itself. This rock consists of bands of somewhat different colours lying parallel to the upper plane, and dipping towards the E at an angle of 45°; these bands differ but little in character, being mostly of the more granular-looking varieties, and considerably hard. Close to this rock are several others of less dimension, but of the same form, and whose upper planes dip at the same angle. It remains to be added that we found the only cleavage practicable by mechanical force, though somewhat obscure, is in this rock parallel to the ordinary cleavage of the slaty rocks of the country, and, therefore, parallel to one of the sides of the prism, visible in the preceding sketch. This, however, is one among a multitude of proofs visible in various parts of the forest, that although no other cleavage is attainable by mechanical means than that which has so often been alluded to, yet a long exposure to the elements produces similar effects in another direction; and hence are to be observed in various places, rhombic prisms of this rock, but the angles at which the planes meet, are by no means constant, and where the rock is not banded, as it is in the above instance, the upper plane is altogether wanting.

In conclusion it may be observed, that although in several instances (some of which have been quoted) these rocks appeared to be stratified parallel to the cleavage plane—that is to say, some of its varieties succeed each other in that direction, yet we are disposed to believe from examination, that the *stratification*, which may be termed *the order in which these rocks were deposited*, is in fact *parallel to the variously coloured bands*, which always are in a direction different to that of the cleavage plane of the slates—never at right angles to it.

It is impossible to view the rocks of Charnwood Forest either *in situ*, or in hand specimens, without being forcibly struck with their resemblance to several of those in many parts of North Wales. The less perfect slates lying between the granular-looking and imperfectly slaty rocks above Nantfrancon quarry, almost perfectly resemble the coarse slates of the Swithland quarries; while in several of the granular varieties of the rocks of the two tracts, the resemblance is absolutely perfect. Chlorite is abundant, and appears to be the colouring matter of both; and both include small crystals of transparent quartz and of felspar, when the rock is very coarsely slaty. In Charnwood Forest, however, we perceived in many places layers containing angular fragments of quartz, and which are left adhering to the surface, owing to the decomposition of the rocks, which in that state, like those of Wales, are of a greyish-white colour. In Wales, however, the included fragments, which alone may suffice to characterize the whole series of Charnwood as belonging to greywacke, have not yet been described as occurring, though from the very great similarity of its rocks and coarser slates to of those the Forest, they may probably hereafter be discovered; for in Wales the fragments hitherto observed in its rocks appear to be chiefly confined to a slaty substance, not unlike flinty slate, and which is actually found *in situ* between layers of the rock itself; the same substance, as well as the nodules which appear to be contemporaneous with the rock, is likewise found in Charnwood Forest.

If, however, the characters of the rocks of some parts of North Wales should hereafter decide them to be greywacke, there are others in which the characters seem so absolutely talcose, that it may be found difficult to assign the whole to one common origin, and equally so to separate them, on account of the close connexion of the whole as regards position and the constant direction of the slaty cleavage through all the varieties. In Wales this direction is about NE and SW, the slates being either vertical, or dip at an angle of about 72° to the NW or SE; while in Charnwood Forest the upper edge of the slate always runs about NW by W, and SE by E, dipping uniformly about 72° a little to the east of N.

Assuming the direction of the variously coloured layers so obvious in many parts of Charnwood Forest, to be that of the stratification, which, perhaps, will scarcely be doubted, and which, as has already been said, differs in different parts of the tract, it may be remarked that this has not been observed in the same degree in North Wales, although it is sufficiently visible near the summit of the Cleweth, a point but little inferior in elevation to that of Snowdon itself.

Rocks of Grooby and its Neighbourhood.

Considering the nature of these rocks, which appear to belong to the trap family, it would have been especially desirable to have perceived their connexion either with those of the forest, or of Mount Sorrel, to neither of which do they seem to possess any affinity; but although we anxiously sought the junction, there did not appear any opportunity of perceiving it. Owing to the causes before mentioned, we did not observe any place where their approach appears on the surface nearer than about one-fourth of a mile, as on the north of Markfield Knowl, which consists of trap. It is remarkable that when this knowl is viewed from the road from Grooby to Markfield, and just before it turns southwards to that place, five ridges of the trap rock are perceived running down its side in the direction of the slaty cleavage of the rocks of Charnwood. We mention this fact as it exists, without any suspicion of its cause, or that these rocks have any connexion with slates of any kind, and especially with those of the forest; the spaces between the ridges are covered by a thick herbage.

About one-fourth of a mile on the north of the lower extremity of these ridges is a quarry beside the road from Grooby to Thrinkston, and called Round Cliff Pit, in which the rocks of Charnwood are broken for the purposes of the road, and which here are traversed by veins of a yellowish-green substance, which, from its hardness, and from the forms of some minute crystals observed in crevices of the rock, we consider to be epidote. The space between this quarry and the termination of the ridges of trap at the foot of Markfield Knowl, is, however, covered by grass, or by cultivated soil, except the small part occupied by the road.

We observed this rock but partially; namely, on the N in Bradgate Park; thence by Grooby Pool to Grooby, and from the latter place by Markfield to Markfield Knowl; but these rocks extend, according to Mr. Greenough's map, S and SW of the latter place, above two miles.

This rock consists primarily of hornblende often distinctly laminated, intermixed with small masses, never exceeding the size of a pea, of a reddish substance, often greatly possessing the aspect of compact felspar, the fractured surfaces being commonly without lustre; these two ingredients constitute the rock in about equal proportions; in other instances the red felspar (if so it may be termed) has a distinct though not brilliant cleavage, but only in one direction. With the reddish substance is often associated another, either of a light or bottle-green colour, of which the surfaces are more brilliant than those of the former, and we succeeded in detaching several fragments, having cleavages at least in two directions, but scarcely bright enough for the use of the reflective goniometer: one of them, however, afforded, by several trials, an angle so very near to one belonging to cleavelandite, and which of course does not belong to felspar, that we have scarcely a doubt of the existence of the former mineral in this rock also; the green crystals are sometimes obviously included in those of a red colour; and we conceive it to be not improbable, that the close intermixture of these two minerals in that which is termed compact felspar, will account for the presence of soda and potash, both of which, it is observed by Dr. Mac Culloch, are constant ingredients of that substance. Both the red and green varieties yield to pressure with a hard mineralogical knife, the green being somewhat the softer of the two, and therefore appear to be less hard than felspar usually is: sometimes this rock contains minute, slender, and transparent crystals, which, from their superior hardness, we consider to be felspar, and specks of green or of yellowish steatite, and oxidulous iron in minute regular octohedrons; quartz is a frequent ingredient; mica and epidote occur more rarely; in the quarry W of Grooby, we observed the rock traversed by veins, in one instance two inches thick, of a hard flesh-coloured substance, which may be cleaved into curvilinear rhomboids, in the cavities of which were minute crystals in that form; and as these crystals, and the substance including them, effervesce slightly, and dissolve slowly in diluted muriatic acid, and as the crystals afforded angles of about 106° 30′ by the reflective goniometer, we conclude them to be magnesian carbonate of lime. The sides of the veins are coated with chlorite, which also is included in the vein itself.

In the quarry on the E of Grooby, we perceived a vein of quartz and chlorite, and some appearance of the rock being traversed by a dyke in a N and S direction, but in too rude a manner for us to ascertain the fact, the walls, or what we conceived to be such, being very irregular and indeterminate, and the substance of the supposed dyke being composed of the same materials as the rock inclosing it, but much finer grained.

Although the connexion of these rocks with those of Charnwood Forest was not perceived, we consider it to be worthy of note, that after descending the south-western slope of Beacon Hill, and traversing a field at its base, we found, beside a small brook, some rocks of very considerable dimension, and which we fully believed to be *in situ*, so perfectly resembling those of Grooby, that it was impossible to doubt their identity of composition; and it may be remarked that fragments of the same rock almost constitute the walls of the neighbouring fields. Whether this rock be a dyke, or in what manner it is connected with the forest rocks of the surrounding elevations, we had no means of ascertaining, since it is the only one which overtops the herbage in this comparatively low situation.

Article II.

On the Crystalline Forms of Artificial Salts,
By H. J. Brooke, Esq. FRS.

(Continued from vol. vi. p. 430.)

It has been suggested to me that these descriptions of the crystalline forms of the salts ought to be accompanied by an exact analysis of each, in order to avoid the chance of misnomer, and to render the description of each complete. It certainly would be more satisfactory to know the composition of the salts described. This cannot, however, be conveniently given with the figures, but I have stated generally the authority upon which they have been severally named, and they may at any time be compared with the forms of ascertained compounds.

Sulphate of Potash.

The primary form of this salt was, I believe, first determined by Mr. Levy to be a right rhombic prism, and described in No. 30 of the Royal Institution Journal; but probably from not possessing sufficiently explanatory crystals, Mr. L. has not pointed out the relation of its primary form to the bi-pyramidal figure under which it generally occurs.

I have been enabled to do this in a very satisfactory manner by means of a compound crystal which I have obtained from the solution of a portion of this salt in distilled water.

Fig. 1.

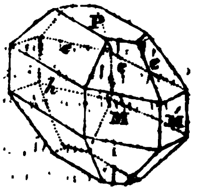


Fig 1. is a single modified crystal.

M on M′	120°	30′
M on *h*	120	45
M on *e*	146	22
h on *c*	146	10
e on *e*′	131	12

Fig. 2.

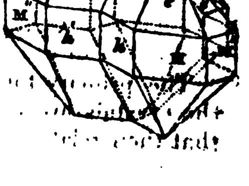
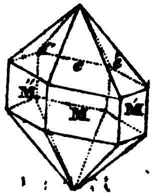


Fig. 2 is the compound crystal, which consists of three single crystals, so united that their upper edges meet at angles of 120°, and consequently their planes of junction incline to each other at the same angle. Hence

M on M″	119°	30′
e on *e*″	130	24

Fig. 3 is one of the common bi-pyramidal crystals, whose relation to the preceding figures may be perceived from the corresponding letters on the planes.

Fig. 3.



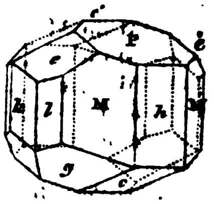
The union of these three crystals at an angle of 120° is a fact which requires some little explanation.

It has been hitherto supposed, that when crystals mutually *intersect* or penetrate each other, the planes of *intersection* are parallel to the primary planes, or to secondary planes resulting from some simple law of decrement. And the theoretic order of arrangement of the molecules of crystals seems to require this mode of combination in *intersecting* crystals. But the union of the three crystals which compose fig. 2 is probably the result of simple accretion, in the same manner as crystals of different substances are found adhering strongly together by the close apposition of their surfaces; with this distinction however between the two cases, that the union of the crystals in fig. 2 is the result of some law which does not govern the accidental union of heterogeneous crystals, nor always even of homogeneous ones.

I have been led to this conclusion relative to the structure of these by-pyramidal crystals, from having observed an *apparently intersected* crystal of chrysoberil, separate easily into six segments, the planes of section being sufficiently bright for measurement with the reflective goniometer, and inclining to each other at an angle of 60°.

Sulphate of Soda.

I do not find any distinct cleavage in the crystals of this salt, whose primary form is *an oblique rhombic prism.*



P on M, or M′	101°	20′
P on *e*, or *e*′	133	18
P on *h*	107	44
P on *c*′	130	45
M on M′	80	24
M on *h*	130	12
M on *l*	162	38
M on *k*	139	48

Nitrate of Lead.—Nitrate of Barytes.

The primary form of these salts is *a regular octahedron,* but the crystals are commonly so much flattened in one direction that their relation to the primary form is not very apparent.

Fig. 1.

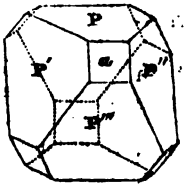


Fig. 2.

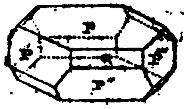


Fig. 1 is the regular primary crystal resting on one of its planes, and having its solid angles truncated.

P on P′	109°	28′
P on *a*	125	16

Fig. 2 is the more common shape of the secondary crystals, resulting from a disproportionate extension of some of their planes.

Article III.

On a new Phenomenon of Electromagnetism. By Sir Humphry Davy, Bart. Pres. RS.*

On a subject so obscure as electromagnetism, and connected by analogies more or less distinct with the doctrines of heat, light, electricity, and chemical attraction, it is not difficult to frame *hypotheses;* but the science is in a state too near its infancy to expect the developement of any satisfactory *theory;* and its progress can only be ensured by new facts and experiments, which may prepare the way for extensive and general reasonings upon its principles. Influenced by this opinion, I am induced to lay before the Society an account of an electromagnetic phenomenon I observed about fifteen months ago in the laboratory of the Royal Institution, and which I have lately had an occasion of witnessing in a more perfect manner, through the kindness of Mr. Pepys, by the use of a large battery, constructed under his directions for the London Institution, and containing a pair of plates of about 200 square feet. In describing this phenomenon, I shall not enter into very minute details, because the experiments, which led to the discovery of it, are very simple, and, though more distinct with a large apparatus, yet it may be observed by the use of a pair of plates containing from ten to fifteen square feet.

Immediately after Mr. Faraday had published his ingenious experiments on electromagnetic rotation, I was induced to try the action of a magnet on mercury connected in the electrical circuit, hoping that, in this case, as there was no mechanical suspension of the conductor, the appearances would be exhibited in their most simple form; and I found that when two wires were placed in a basin of mercury perpendicular to the surface, and in the voltaic circuit of a battery with large plates; and the pole of a powerful magnet held either above or below the wires, the mercury immediately began to revolve round the wire as an axis, according to the common circumstances of electromagnetic rotation, and with a velocity exceedingly increased when the *opposite* poles of two magnets were used, one above, the other below.

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* From the Philosophical Transactions for 1823, Part II.

Masses of mercury of several inches in diameter were set in motion, and made to revolve in this manner, whenever the pole of the magnet was held near the perpendicular of the wire; but when the pole was held above the mercury between the two wires, the circular motion ceased; and currents took place in the mercury in opposite directions, one to the right, and the other to the left of the magnet. These circumstances, and various others which it would be tedious to detail, induced me to believe that the passage of the electricity through the mercury produced motions independent of the action of the magnet; and that the appearances which I have described were owing to a composition of forces.

I endeavoured to ascertain the existence of these motions in the mercury, by covering its surface with weak acids; and diffusing over it finely divided matter, such as the seeds of lycopodium, white oxide of mercury, &c. but without any distinct result. It then occurred to me, that from the position of the wires, currents, if they existed, must occur chiefly in the lower, and not the upper surface of the mercury; and I consequently inverted the form of the experiment. I had two copper wires, of about one-sixth of an inch in diameter, the extremities of which were flat and carefully polished, passed through two holes three inches apart in the bottom of a glass basin, and perpendicular to it; they were cemented into the basin, and made non-conductors by sealing-wax, except at their polished ends; the basin was then filled with mercury, which stood about a tenth or twelfth of an inch above the wires. The wires were now placed in a powerful voltaic circuit. The moment the contacts were made, *the phenomenon*, which is the principal object of this paper, occurred: the mercury was immediately seen in violent agitation; its surface became elevated into a small cone above each of the wires; waves flowed off in all directions from these cones; and the only point of rest was apparently where they met in the centre of the mercury between the two wires. On holding the pole of a powerful bar magnet at a considerable distance (some inches) above one of the cones, its apex was diminished and its base extended: by lowering the pole further, these effects were still further increased, and the undulations were feebler. At a smaller distance the surface of the mercury became plane; and rotation slowly began round the wire. As the magnet approached, the rotation became more rapid; and when it was about half an inch above the mercury, a great depression of it was observed above the wire; and a vortex, which reached almost to the surface of the wire.

In the first experiments which I made, the conical elevations or fountains of mercury were about the tenth or twelfth of an inch high, and the vortices apparently as low; but in the experiments made at the London Institution, the mercury being much higher above the wire, the elevations and depressions were much more considerable, amounting to the fifth or sixth of an inch. Of course, the rotation took place with either pole of a magnet or either wire, or both together, according to the well-known circumstances which determine these effects.

To ascertain whether the communication of heat diminishing the specific gravity of the mercury, had any share in these phenomena, I placed a delicate thermometer above one of the wires in the mercury, but there was no immediate elevation of temperature; the heat of the mercury gradually increased, as did that of the wires; but this increase was similar in every part of the circuit. I proved the same thing more distinctly, by making the whole apparatus a *thermometer* terminating in a fine tube filled with mercury. At the first instant that the mercury became electromagnetic, there was no increase of its volume.

This phenomenon cannot be attributed to common electrical repulsion; for in the electromagnetic circuit, similar electrified conductors do not repel, but attract each other; and it is in the case in which conductors in *opposite* states are brought near each other on surfaces of mercury, that repulsion takes place.

Nor can the effect be referred to that kind of action which occurs when electricity passes from good into bad conductors, as in the phenomena of points electrified in air, as the following facts seem to prove. Steel wires were substituted for copper wires, and the appearances were the same in kind, and only less in degree; without doubt, in consequence of a smaller quantity of electricity passing through the steel wires: and by comparing the conducting powers of equal cylinders of mercury and steel in glass tubes, by ascertaining the quantity of iron filings they attracted, it was found that the conducting powers of mercury were higher than those of steel; the first metal taking up fifty-eight grains of iron filings, and the second only thirty-seven.

Again; fused tin was substituted for mercury in a porcelain vessel into which wires of copper and steel were alternately ground and fixed: the elevations were produced as in the mercury, and the phenomena of rotation by the magnet; and it was found by direct experiment, that the conducting powers of the tin, at and just before its point of fusion, did not perceptibly differ, and that they were much higher than those of mercury. Lastly, the communication was made from the battery by two tubes having nearly the same diameter as the wires filled with mercury, so that the electricity, for some inches before it entered the basin, passed through mercury; and still the appearances continued the same.

From the rapidity of the undulations round the points of the cones, I thought they would put in motion any light bodies placed above the mercury; but I could not produce the slightest motion in a very light wheel hung on an axle; and when fine powders of any kind were strewed upon the surface, they merely underwent undulations, without any other change of place; and fine iron filings strewed on the top of the cone, arranged themselves in right lines at right angles to the line joining the two wires, and remained stationary, even on the centre of the cone. The effect, therefore, is of a novel kind, and in one respect seems analogous to that of the tides. It would appear as if the passage of the electricity diminished the action of gravity on the mercury. And that there is no change of volume of the whole mass of the mercury appears from the experiment, p. 24; and this was shown likewise by inclosing the apparatus in a kind of manometer, terminating in a fine tube containing air inclosed by oil; and which, by its expansion or contraction, would have shown the slightest change of volume in the mercury: none however took place when the contacts were alternately made and broken, unless the circuit was uninterrupted for a sufficient time to communicate sensible heat to the mercury.

This phenomenon, in which the same effects are produced at the two opposite poles, seems strongly opposed to the idea of the electromagnetic results being produced by the transition currents or motions of a single imponderable fluid.

On the conjectural part of the subject I shall not however enter, for the reasons stated in the beginning of this paper; but I cannot with propriety conclude, without mentioning a circumstance in the history of the progress of electromagnetism, which, though well known to many Fellows of this Society, has, I believe, never been made public, namely, that we owe to the sagacity of Dr. Wollaston, the first idea of the possibility of the rotations of the electromagnetic wire round its axis, by the approach of a magnet; and I witnessed, early in 1821, an unsuccessful experiment which he made to produce the effect in the laboratory of the Royal Institution.

ARTICLE IV.

A Reply to some Observations in the Review of An Essay upon the Constitution of the Atmosphere. By J. F. Daniell, FRS.

(To the Editor of the *Annals of Philosophy.*)

MY DEAR SIR, *Gower-street, Dec. 10, 1823.*

In the review of my Essay upon the Constitution of the Atmosphere contained in the last number of the *Annals*, your correspondent Z appears to me to have fallen into some misconceptions which I should be sorry to leave uncorrected in a work of so much authority. I therefore trust that you will do me the favour to spare me room for a short reply.

In the first place, it is objected to my theory, that "it rests upon the sandy foundation of assumed partial changes of temperature in the higher regions of the atmosphere, of the existence of which we have very insufficient evidence." This assertion, I must own, greatly astonishes me: for I certainly conceived that no meteorological fact rested upon better authority. I thought it indeed to be so generally admitted, that I did not refer in my Essay to the particular observations upon which it is founded, but merely illustrated it by the observation of De Luc, of the sudden rise of temperature accompanying the formation of cloud at a great elevation. I might now appeal, I believe, to every ascent of every mountain, and every aërostatic voyage, during which the thermometer has been consulted; for they all appear to me to agree in the same result. Among the latter, more particularly, there are abundant instances in which, not only the temperature of the atmosphere has not followed the regular progression due to the density, but warm strata have been found interposed between cold, and cold between warm. I shall content myself, however, at present, with referring Z to the simultaneous registers kept at the summit of St. Bernard and at Geneva, and which are published every month in the Bibliothèque Universelle. He will there find that the difference of temperature between the two stations is perpetually varying, and that, although the changes oscillate round the point of equilibrium, the general law of the decrease for the altitude is only developed from a mean of many observations.

The second objection is, "that to evolve so much heat as would raise the temperature of a considerable mass of air, and cause it to diffuse itself rapidly into distant regions, would require the condensation of a greater quantity of aqueous vapour than is likely to be present in any given space, and also that this condensation should not be gradual, but should take place suddenly to a very great amount." From this extract I am almost tempted to believe that Z has not done me the honour to read my Essay, but *only to review* it. I have taken much pains to show at p. 31, *et seq.* that such a sudden condensation of a large quantity of vapour is not by any means requisite to produce an effect upon the barometer, but on the contrary, that a gradual partial increase of temperature in the strata of an atmospheric column of only two degrees is sufficient to depress the mercury 0·63 in. If this part of my illustration should not have been overlooked, I can only at present join issue upon the opinion; pledging myself to examine impartially and thankfully any argument which Z may hereafter bring against my conclusions.

My reviewer objects in the third place to an error into which not only myself but Mr. Leslie has fallen, *viz.* that the particles of air in passing over the surface of the globe do not for a moment cease to gravitate, and that no horizontal movement of them will produce the slighest derangement in a perpendicular direction. He observes, "Now it is well known that any body, to which a projectile motion of five miles per second has been imparted, would revolve around the earth like a planet, and *would cease to exert any pressure on its surface.* Any less velocity must produce a proportional decrease of weight in the particles of air, which is known to move at the rate of from 60 to 100 miles per hour."

As this argument, if correct, is indeed decisively subversive of my theory, I trust that I may be allowed to illustrate it somewhat particularly. According to the proposition set forth by Z, a loaded waggon presses less upon the earth when at rest than when in motion upon the road! A steam boat must rise out of the water in proportion to the velocity communicated to it by its engine!! And many vessels must doubtless have been upon the point of upsetting from this ill-understood cause!!! To these bodies, it is true, we cannot communicate a projectile motion of five miles per second, but the former will move about five miles per hour, and the latter probably ten, and "these lesser degrees of velocity must produce a proportional decrease of weight." Why a cannon ball indeed when projected from a gun does not mount in the air like a soap-bubble, is not quite obvious; for such a result one might undoubtedly expect from the theory of Z. But to be serious:—With a laudable desire of correcting my blunders, it is clear that the reviewer has fallen himself into an error of no trivial importance. It is not true that any body to which a projectile force of five miles per second has been imparted, would revolve around the earth like a planet, *unless this motion were to take place in vacuo, and were unopposed by any pressure whatever:* and no body that ever pressed at all upon the surface of the earth would cease to exert such pressure in consequence of any degree of projectile motion that might be communicated to it. It is not for me to surmise in what way it can have been possible for your correspondent to confound together pressure and gravitation, but certain it is that the planets do not press upon the sun, nor the moon upon the earth. It is no less certain that all the particles of the atmosphere do press upon the earth, and, like the waggon upon the road, the vessel upon the water, or the cannon ball in the air, their weight is in no degree dependant upon their motion or rest.

These are the only objections brought forward by Z against my theory of the Constitution of the Atmosphere; but he "suggests that the third table in Part I. is founded upon an erroneous principle." In reply to this, I must observe that the tables in this part of my work were not meant to be accurate representations of the different states of the atmospheric columns, but mere rough approximations; and their use is to assist the mind in following the train of reasoning in the same way that rudely-sketched diagrams assist the mathematician in solving a problem of Euclid. The principle upon which they were constructed is this: I assumed the mean temperature of the latitude for which I wished to calculate the atmospheric column as the temperature of an ~~homogeneous atmosphere~~; and I thence derived the pressures at different altitudes from the surface, and from these the regular decrease of temperature for the density. It is clear that for accurate purposes, both the pressures and temperatures so obtained require correction, and that the tables include an error which should be divided between the two, and does not fall wholly upon the pressure as suggested by Z. The labour of applying these corrections would have been very considerable; and the tables, which, even in their present state, cost me much pains, would not have better answered the purposes of illustration.

In conclusion, I cannot but express my acknowledgments to Z for the general courtesy of his review, but am still inclined to appeal from his tribunal; for I must own that my ambition cannot yet content itself with the somewhat meagre consolation which has been offered to me, that "there can be no discredit to any one who fails to unfold the causes of phenomena which have been acknowledged by one of the first philosophers of the present times to have hitherto baffled all attempts to reduce them to fixed principles."

I remain, dear Sir, yours most faithfully,

J. F. Daniell.

ARTICLE V.

Astronomical Observations, 1823.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Dec. 7.	Immersion of Jupiter's third satellite	9h	24′	41″	Mean Time at Bushey.
		9	26	02	Mean Time at Greenwich.
Dec. 12.	Immersion of Jupiter's first satellite	12	21	19	Mean Time at Bushey.
		12	22	40	Mean Time at Greenwich.
Dec. 12.	Immersion of Jupiter's second satellite	14	17	04	Mean Time at Bushey.
		14	18	25	Mean Time at Greenwich.

A grievous error must exist in the time set down for the emersion of Jupiter's third satellite, Dec. 7.

According to the Nautical Ephemeris, it should have taken place at 12h 27′ 01″; but after fruitlessly watching at the telescope until 14h 20′, that is for more than two hours after the time specified, clouds relieved me from further attendance.

ARTICLE VI.

On Lupulin, as a Medicine. By Nicholas Mill, Esq.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Bridge Cottage, Camberwell, Nov. 20, 1823.*

HAVING noticed in an American journal the experiments of Dr. Ives at New York on the annon hop, and which appeared to me of considerable importance in medicine, I was induced to extend those inquiries, and apply the result, if beneficial, to practice among my own particular friends.

Preparations of the hop have been occasionally used in medicine in this country. The whole of the plant has usually been employed to form a tincture, but from the extraneous matter introduced by this means, it has doubtless rendered this medicine inert, if not prejudicial. Dr. Ives discovered that the true aromatic bitter of the hop resided solely in a pulverulent matter, which he called lupulin, for the collection, preparation, and administration of which I am about to give specific directions.

Take any quantity of the best hops, and rub them strongly between the hands, or put them in a bag, and beat them for some time; when the beating is completed, throw them on a coarse wire sieve, which will only suffer the dust, &c. to pass it; let them be well rubbed on the sieve till every thing has gone through, except the leaves and stems of the plant; reject the leaves and stems altogether, and sift what has already passed the wire through a lawn sieve; nothing will now pass but a very fine powder resembling red sand: this is the lupulin in which the whole virtue of the hop resides.

The preparations of this substance which I have found to be most efficacious are the decoction and tincture.

The decoction may be made by putting a sufficient quantity of lupulin into a Florence flask, in a sand heat, and filling it three parts full with distilled water; boil the whole for half an hour, and strain through cotton cloth. The solution thus obtained will be feculent, and does not become clear by repose; therefore add, while hot, a small quantity of solution of gelatine in hot water; shake the whole together, and let it remain till cold, then filter through paper, and a clear yellow liquid will be obtained. It is intensely, but not unpleasantly bitter; and when administered in doses of a tea-spoonful at a time in a table spoonful of cold water, is a true stomachic. It is tonic, narcotic, and aromatic. It does not produce constipation of the bowels, as almost all other tonics do. It appears to act entirely on the nervous system, and may be prescribed with manifest advantage in all cases of debility and inaction of the digestive organs where powerful tonics would be injurious.

The tincture may be prepared by digesting the lupulin in *strong and warm alcohol*, till saturated, when it must be filtered through paper, and a deep-red solution will be obtained.

From 40 to 60 minims of this tincture act as an anodyne, and have a powerful effect in allaying great nervous irritation; and that stupidity which often accompanies the use of opium, is never induced by this medicine.

I am, Sir, your obedient servant,

Nicholas Mill.

Article VII.

On the Methods of employing the various Tests proposed for detecting the Presence of Arsenic. By R. Phillips, FRS. &c.

So much has at various times been offered to the public on the subject of ascertaining the presence of arsenic, whether taken from the stomachs, or mixed with the food of those who have fallen victims to its poisonous agency, that some apology may seem requisite for resuming the subject, more especially as it has lately been treated of in so able a manner by Dr. Paris, in his work on Medical Jurisprudence. My object on the present occasion is not to offer any new test for arsenic: what I propose is, to modify some of the methods of using the means already known, and to simplify their application. I shall not enter into any history of the various means which have been proposed, but confine myself to treating of the mode of employing the following tests; viz. the reduction of the arsenious acid to its metallic state; the use of sulphuretted hydrogen; sulphate of copper; and nitrate of silver; and I hope to be able to describe the little apparatus required in such a manner as to remove some ambiguity and difficulty.

It is justly observed by Dr. Paris (Jurisprudence, vol. ii. p. 252), that "the detection of the presence of arsenic amidst a complicated mass of alimentary matter has long been a problem of interest and difficulty," because "coloured fluids are capable of obscuring and changing, and even altogether preventing the arsenical indications." Dr. Paris then adverts to the proposal of M. Orfila to destroy or modify the colouring matter by means of chlorine; this method is justly stated to be liable to great difficulties in its application, and from sources too obvious to require notice. Dr. Paris, therefore, advises that a solution of ammoniuret of silver should be added to the fluid to precipitate indiscriminately all bodies which it may be capable of so affecting; and then subjecting the precipitate to the action of black flux in a glass tube for the purpose of subliming the arsenic in its metallic state.

On considering this part of the subject, it appeared to me that animal charcoal (ivory-black) might be advantageously employed for the purpose of destroying the colouring matter. I therefore, mixed some of it with a coloured solution of arsenious acid, viz. the *liquor arsenicalis* of the London Pharmacopœia. I found that the colouring matter was so completely destroyed in a few minutes, that the test of nitrate of silver, or any other, might be readily applied. I repeated this experiment with port wine, gravy soup, and a strong infusion of onions [*], and succeeded in these cases in procuring a solution sufficiently colourless for the application of the most delicate tests. It might be supposed that the phosphoric acid which the animal charcoal contains might have some share in the production of the yellow precipitate with silver; I found, however, that water or wine which was merely digested on the animal charcoal, produced no effect with the nitrate of silver, excepting a slight precipitate of chloride; and to prevent this, the ivory-black should be washed on a filter with boiling distilled water, till the fluid running through ceases to be affected by nitrate of silver. If, however, the ivory-black is of good quality, this precaution is unnecessary.

Of Sulphuretted Hydrogen.

Supposing the substance suspected to contain arsenious acid, to have been boiled in distilled water without any alkali, and deprived of its colouring matter as now described, the test to which it may at first be subjected is a solution of sulphuretted hydrogen gas in water. This is to be preferred to any coloured fluid containing sulphuretted hydrogen, such as the hydrosulphuret, or, perhaps more properly, hydroguretted sulphuret of ammonia; for this fluid possesses the colour when diluted, which we expect to produce by the action of sulphuretted hydrogen upon arsenious acid. The method of preparing this solution of the gas is perfectly simple. Put into an oil flask about two ounces of muriatic acid undiluted with water, and an ounce and a half of powdered sulphuret of antimony; fit a cork to the flask, and pass through it the shorter leg of a small hollow glass tube twice bent at right angles; pass the longer leg of the tube into a vial containing distilled water, and then by the heat of a spirit-lamp applied to the flask, sulphuretted hydrogen gas will be readily procured, and though much of it will escape, yet a sufficient quantity will be dissolved by the water. If distilled water cannot be procured, then rain water may be used, or if that be not at the moment obtainable, water, which has been boiled and become clear, must be substituted. The annexed cut will sufficiently explain this apparatus.

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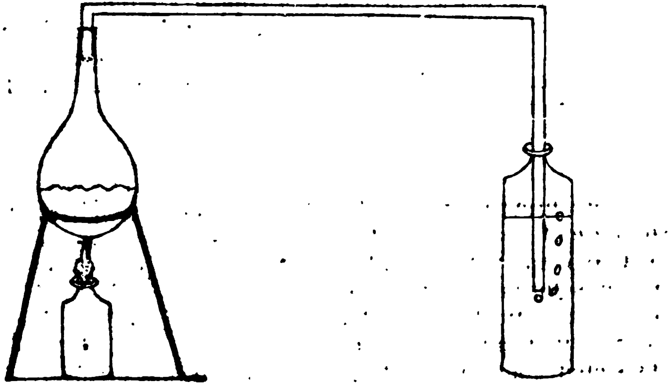
[*] I particularly mention infusion of onions, because much stress has been laid upon the ambiguity which its yellow colour may occasion. It immediately becomes colourless. (See Paris and Fonblanque's Med. Jurisprudence, vol. iii. p. 190.)



If a glass tube is not at hand, one may be readily formed of tin plate soldered. I have employed one for the purpose of trying the experiment, which was made in about an hour; the shorter leg may be about two inches and a half to three inches long; the intermediate space, and the longer leg, each six inches in length; the diameter may be about one-fourth of an inch; this passed through the cork, and used instead of the glass one, answers the purpose perfectly, notwithstanding the tin is slightly acted upon by the gas during its passage, and by the solution in which it is immersed.

To this solution, clear and colourless, add, in a wine glass, or a vial, some of the suspected fluid; if it contain arsenious acid, a yellow-coloured fluid will be produced, and, after the lapse of some hours, a yellow precipitate will fall down. It has been objected to this test, that antimony produces a similar appearance; while, however, there exists some resemblance in the colours, there are these differences in the effects; antimony furnishes a precipitate immediately, and with more of an orange tint.

Sulphate of Copper.

Arsenious acid produces no effect upon a solution of sulphate of copper; but with the assistance of an alkali, a green precipitate of arsenite of copper is readily produced. It has been objected to this test, that a fallacious appearance is produced if the suspected solution be of a yellow colour; but for this case I have already provided. If the sulphate of copper be impure, owing to the presence of peroxide of iron, a greenish precipitate may also be obtained by simply adding potash to the solution. This test may be employed in two ways; first, add a few drops of an alkaline solution, as potash or its *sub*-carbonate to the suspected solution, and when mixed pour them into the sulphate of copper. If arsenious acid be present, a green precipitate will be formed; and there is a mode of removing any ambiguity which, if it does not escape my recollection, has not been previously noticed. To be certain that the sulphate of copper contains no peroxide of iron, add first to the solution some potash; if pure, a fine blue precipitate will be obtained; to this add the suspected solution; and if arsenious acid be present, then it will convert the blue precipitate to a green one.

Nitrate of Silver.

To confirm the results obtained by sulphuretted hydrogen and sulphate of copper, nitrate of silver is a test which may be very usefully employed. The first method is simply to add the suspected solution to one of nitrate of silver, which should be prepared either from the crystallized or fused nitrate (lunar caustic), in order that all excess of acid may be avoided. After the suspected solution has been mixed with that of silver, drop in a solution of ammonia or of potash, liquor ammoniæ, or liquor potassæ of the London Pharmacopœia; if arsenious acid be present, a bright-yellow precipitate of arsenite of silver will be formed, which is readily dissolved by excess either of ammonia or nitric acid; so that supposing too much ammonia to have been employed, nitric acid will restore the precipitate. Potash does not possess the inconvenience of ammonia in redissolving the arsenite of silver formed: but there is one inconvenience attending the use of silver; the animal fluids all contain muriatic salts; and, therefore, the fluid contents of the stomach will probably give a white precipitate of chloride of silver when mixed with the nitrate. If, however, the presence of arsenic has been determined by the use of the preceding tests, then the chloride and arsenite of silver must be precipitated together, and the mixture, after being dried, must be subjected to the metallizing process, to be described presently.

Nitrate of silver is liable to ambiguity, and on this subject I cannot do better than quote what Dr. Paris has stated in the work already alluded to, vol. ii. p. 241.

"The alkaline phosphates are found to produce precipitates with silver, analogous in colour and appearance to the arsenite of silver. This constituted one of the principal points in the evidence for the defence, on the trial of Donnall for the murder of Mrs. Downing; and it must be admitted as a valid objection, if the experiment be performed in the manner just stated; but there are other reagents which will immediately distinguish these bodies, as we shall presently have occasion to state, under the history of the ammoniuret of silver, as a test for arsenic. The author has also shown, that there is a mode of so modifying the application of the present test, that no error or doubt can arise in the use of it, from the presence of any phosphoric salt. This method consists in conducting the trial on writing paper, instead of in glasses; thus—drop the suspected fluid on a piece of white paper, making with it a broad line; along this line a stick of lunar caustic is to be slowly drawn several times successively, when a streak is produced of a colour resembling that known by the name of Indian yellow; and this is equally produced by the presence of arsenic, and that of an alkaline phosphate, but the one from the former is rough, curdy, and flocculent, as if effected by a crayon, that from the latter is homogeneous and uniform, resembling a water-colour laid smoothly on with a brush; but a more important and distinctive peculiarity soon succeeds, for in less than two minutes the phosphoric yellow fades into a sad green, and becomes gradually darker, and ultimately quite black; while, on the other hand, the arsenical yellow remains permanent, or nearly so, for some time, when it becomes brown. In performing this experiment, the sunshine should be avoided, or the transitions of colour will take place too rapidly. It would be also prudent for the inexperienced operator to perform a similar experiment on a fluid known to contain arsenic, and on another with a phosphoric salt, as a standard of comparison."

The ambiguity arising from the use of nitrate of silver has also been most satisfactorily obviated by Mr. Smithson (*Annals of Philosophy*, Aug. 1822). This method consists in converting the arsenious into arsenic acid, or rather into arseniate of potash; and Mr. S. observes, "that a drop of a solution of oxide of arsenic in water, which at a heat of 54·5° of Fahr. contains not above 1-80th of oxide of arsenic, put to nitrate of potash in the platina spoon and fused, affords a considerable quantity of arseniate of silver. Hence when no solid particle of oxide of arsenic can be obtained, the presence of it may be established by infusing in water the matters contained in it."

Instead of using a platina spoon, a glass tube, or the bottom of an oil flask, may be employed; into either of these, put a little of the suspected solution, and which has exhibited indications of the presence of arsenic by other tests; then drop in a small crystal of nitre, evaporate the solution to dryness by means of a spirit-lamp, and afterwards heat it strongly in the same way. Add a little distilled water to the residuum, dissolve it, and then add nitrate of silver; if the solution before heating contained *arsenious* acid, it will now contain *arseniate* of potash, which will give a brick red precipitate with the nitrate of silver, and without the intervention of any alkali. From repeated trials, I consider the confirmatory evidence afforded by this experiment as amounting almost to demonstration. This experiment is rendered shorter, and not less conclusive, by employing the arsenious acid and nitre both in the state of powder; but as the former is not always procurable after fatal effects have been produced by it, I have mentioned the solution as affording very satisfactory results.

I shall now mention the method of confirming the previous experiments by reducing the arsenious acid to its metallic state. If the quantity of arsenious acid procurable be very small, then it is proper to dissolve the whole of it in distilled water, and the precipitates which are obtained by the action of the various reagents should be collected and submitted to the metallizing process; but if the quantity of arsenious acid be so large that a few grains, or not less than one grain, can be spared for metallization, then the precipitates may be rejected, and much trouble will be spared.

This process is thus recommended to be performed by Dr. Paris, in his work before alluded to, vol. ii. p. 233:—

"Mix a portion of the suspected substance in powder, with three times its weight of black flux;* put the mixture into a thin glass tube, about eight inches in length, and a quarter of an inch in diameter, and which is hermetically sealed at one end. Should any of the powder adhere to the sides of the tube, it must be carefully brushed off with a feather, so that the inner surface of its upper part may be perfectly clean and dry. The closed end of the tube, by way of security, may be thinly coated with a mixture of pipe-clay and sand; but this operation is not absolutely necessary. The open extremity of the tube is to be loosely plugged with a piece of paper. The coated end must now be submitted to the action of heat, by placing it in a chaffing dish of red-hot coals, for ten minutes, or a quarter of an hour; when, if our supposition respecting the nature of the substance has been correct, metallic arsenic will sublime, and be found lining the upper part of the tube with a brilliant metallic crust. The glass tube, when cold, may be separated from its sealed end by the action of a file, which will enable us to collect and examine the metallic sublimate. If a portion of this brilliant matter be laid on heated iron, it will indicate its nature by exhaling in dense fumes, having a powerful smell of garlic. Another portion should be reserved for future experiments.

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* This substance may be said to consist of charcoal in a state of extremely minute division, and the subcarbonate of potash. It is prepared by deflagrating, in a crucible, two parts of supertartrate of potash with one part of nitrate of potash.

"This method of detecting the presence of arsenious acid has been considered the most decisive, and indeed the only unexceptionable one, but of this we shall speak hereafter; at present we have only to observe, that it is very far from being a minute test; for Dr. Bostock confesses that where less than three-fourths of a grain were used, he could not say that the metallic crust was clearly perceptible; and Dr. Black appears to have considered that one grain was the smallest quantity which could be distinctly recognised by such a process."

This method is unquestionably excellent, but I have found that the metallization may be very conveniently effected by means of a spirit-lamp. Indeed it may possibly happen that a glass tube, such as is requisite for the above process, cannot be procured at the moment in which it may be wanted; a spirit-lamp may also be wanting. I have adopted the following plan: let a piece of tin plate, about an inch long, be coiled up into a cylinder of about 3-8ths of an inch in diameter, and if the edges be well hammered, it is not necessary to use solder. Perforate a cork previously fitted to a vial, and put a cotton wick through the short tin tube, and the tube through the cork, the lamp is now complete, and will afford a strong flame, taking care of course not to prevent the rise of the spirit by fitting the cork too closely. Instead of a test-tube about six inches in length, which however is certainly much to be preferred, I have employed, with the precautions copied from Dr. Paris, a common draught vial; those best adapted for the purpose are called *ten drachm* vials, for they are long in proportion to their diameter. In using these vials, the suspected powder and black flux must not reach the bottom of the vial, for, on account of its thickness, it will readily break on the application of heat. The vial, therefore, must be heated laterally, the arsenic will readily sublime, and will, after the vial has been divided by a file, if heated in the spirit-lamp, give out the well-known alliaceous smell. Indeed if the quantity of arsenious acid be large, the smell which the volatilised metal affords may be resorted to in confirmation of other evidence; but it is to be observed, that it must be mixed with charcoal, or some substance which reduces it to the metallic state, for arsenious acid, though volatilized by heat, and exhibiting white fumes, does not give any smell.

I have now concluded the sketch which I proposed giving, and, if I mistake not, the use of animal charcoal in the mode described, will afford some facilities. I hope also that I have, in some degree, strengthened the evidence which is afforded by using sulphate of copper, and rendered the process of metallization less difficult by using common instruments, and such as are within the reach of every practitioner, or readily procurable by him. In concluding, I beg to refer the reader to the work on Medical Jurisprudence, to which I have been so largely indebted, as one which will afford him much and minute information on a subject of some difficulty, and of great importance.

ARTICLE VIII.

Corrections in Right Ascension of 37 *Stars of the Greenwich Catalogue, together with an Inquiry how far it would be advisable that the Daily Corrections in R.A and North Polar Distance of the* 46 *Zero Stars should be computed Annually at the Public Expence.* By James South, FRS.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Blackman-street, Dec. 18, 1823.*

HAVING for some years principally devoted myself to the pursuit of practical astronomy, I have seen with much regret the various difficulties which the private observer has to contend with; having also severely felt some of them, I have endeavoured occasionally to diminish them for others: knowing also that some of these auxiliaries have been used in the most important observatories in the country, and that to some private individuals they have proved welcome, in the absence of others having stronger claims to confidence, I am induced to publish the corrections in right ascension of the 37 stars of the Greenwich catalogue, for every day of the year 1824.

I had indulged a hope (as a reference to this journal two years ago will prove) that the daily corrections not only in Right Ascension, but also in North Polar distance, not only of the 37, but of the 46 Zero stars, would long since have made their appearance, under the sanction of a Society instituted expressly for the purpose of promoting astronomical science. As, however, these hopes are not realised, owing probably to the little want which most of its leading members have of such a publication, it may be worth while to see whether a case cannot be made out sufficiently strong, to justify government in having such corrections computed, at the *public expence.*

Our inquiry will then be divided into three parts; first, *what will be the probable benefits resulting from such a publication*; secondly, *what the expence of procuring it*; and lastly, *how far the former is equivalent to the latter.* In doing this it will be necessary to enter somewhat minutely into the matter, as there are many individuals, although perfectly conversant with the principles of astronomy, who have little idea of the routine of observatory business.

The province of the practical astronomer is to determine the apparent place of all sidereal bodies which come within the reach of his instruments, and to observe such phenomena as from time to time present themselves; in the present instance, we shall confine ourselves to the former. It is scarcely necessary to mention, that by the place of any body in the sidereal heavens is understood its right ascension and north polar distance; each is determined generally at the moment in which the object passes the meridian of the observer, by the aid of instruments fixed in its plane; the transit instrument (with its appendage, the clock) giving the former, while the quadrant or circle indicate the latter.

But by the successive labours of Bradley, Maskelyne, and Pond, the places of 46 stars have been determined with extreme accuracy, these we consider as Zero points when we would assign to any celestial body, its right ascension or north polar distance. Accordingly the business of the practical astronomer among us, as far as right ascensions are concerned, is to secure the meridian passage of each of these stars, or as many of them as possible, and also of as many other stars, planets, or comets, as opportunity will allow; he then finds the error of his clock by each Zero star, at the time of observation, thence deduces its mean error at a corresponding time; he next determines the clock's daily rate by comparisons with previous observations of the same stars; and hence obtains a mean rate. With these materials he is now prepared, by the aid of a little calculation, to apply the clock's error to each observed transit, and is thus furnished with the observed right ascension of each sidereal object at the time of its passing the meridian of his observatory.

Of all these calculations, however, that whereby he arrives at the error of his clock is by far the most troublesome; for before he can find its error by a single star, he must apply corrections to the star's mean right ascension, brought up to the 1st of Jan. of the current year; and these he must seek by reference to the 17th and 18th tables of Dr. Maskelyne's; the first of which gives him the sum of the corrections for aberration, precession, and solar inequality of precession. Nine times out of ten, however, he has to find the equation by proportional calculation, and when gotten, it is sometimes positive, sometimes negative. Having proceeded thus far, he refers to the Nautical Almanac for the place of the moon's node, and consults Table 18, which affords him, *rarely* without calculation, the correction for lunar nutation; again sometimes a positive, sometimes a negative quantity: he now applies one correction to the other, and procures a result which, added to or subtracted from the star's mean right ascension, affords him the star's apparent right ascension at the time required; and which compared with the observed transit, presents him with the clock's error, by that particular star. Thus has he to hunt out corrections for every one of the 36 stars before he can convert its observation to any useful purpose; and

I know by experience, that less than three minutes will not suffice to procure with care, the correction in right ascension for each star: and he must have little experience, or less candour, who will not acknowledge that, with all his circumspection, he has not occasionally taken out a false quantity from a wrong column, or applied one correction to the other with a wrong sign.

But it may be said, a reference to preceding observations will immediately detect the error: not so perhaps; many days may have elapsed since a transit of the same star may have been observed; or it may be urged, that the amount of error, should it escape unnoticed, will be such as not materially to invalidate the result. Now as far as small instruments, such as are usually stuck out of a window, are concerned, I will concede the point, for with these, an erroneous computation, amounting to two or three-tenths of a second, may really do no harm; but where an instrument is used, adequate under favourable circumstances, to assign to any star south of our zenith, its right ascension by a *single* observation, accurate to the largest of these quantities, an error in the calculation of the correction becomes extremely injurious; for it may so far vitiate others, as to require many additional observations to invalidate its force. We shall then, perhaps, be told, reject it when reduced to the 1st day of the year; this may certainly be done, but I hold it a bad principle to discard any observation, unless posted as bad at the time of entering it in the rough journal; it leads to temptation which ought *in limine* to be checked. Observations, be it never forgotten, are not less entitled to our confidence because they are not always uniform; and were I asked why the observations made at our Royal Observatory have acquired the influence they have over Europe, I should reply, not only because its instruments are superior, but because every observation, good, bad, and indifferent, which has been entered in the Observatory Journal, has been honestly recorded in the printed copies.

The remaining process of computing is extremely simple. When, however, 50 or 60 stars are observed daily, its irksomeness is quite sufficient; a circumstance which induced me some time since to remove as much of the drudgery as could be removed, by computing a table, in which the clock's daily rate and error at a particular time of the day being known, its corresponding error at any given time might be found by *inspection.*

(*To be concluded in our next.*)

The *accompanying Corrections* are computed from Dr. Maskelyne's tables, except those of the pole star, which are derived from its apparent right ascension, given in the Nautical Almanac for 1824. For the mean right ascensions, I am indebted to the kindness of the Astronomer Royal.

Note.—It being generally admitted that something in the shape of an Astronomical Ephemeris is much needed, I shall publish in the Journal of Science and the Arts for January next, a list of astronomical phenomena arranged in order of succession for the first three months of the year 1824.

	γ Pegasi	Polaris	α Arietis	α Ceti	Aldebaran	Capella	Rigel	β Tauri	α Orionis
Mean AR 1824.	h. m. s. 0 4 11·12	h. m. s. 0 58 2·66	h. m. s. 1 57 16·42	h. m. s. 2 53 5·44	h. m. s. 4 25 50·61	h. m. s. 5 3 42·21	h. m. s. 5 6 5·16	h. m. s. 5 15 10·52	h. m. s. 5 45 36·93
Jan. 1	+ 0·82″	+ 1·09″	+ 1·57″	+ 1·80″	+ 2·32″	+ 3·21″	+ 2·24″	+ 2·69″	+ 2·41″
2	81	0·39	56	79	32	21	24	69	41
3	80	− 0·31	55	79	32	21	24	69	42
4	79	1·01	53	78	31	21	24	69	42
5	78	1·70	52	77	31	21	24	69	43
6	77	2·38	51	76	31	21	24	69	43
7	75	3·06	50	76	30	21	23	69	44
8	74	3·74	49	75	30	20	23	70	45
9	73	4·42	47	75	29	20	23	70	45
10	71	5·11	45	74	29	20	23	70	46
11	70	5·82	44	73	28	19	23	70	46
12	69	6·52	43	72	28	19	22	69	46
13	68	7·23	41	71	27	18	22	69	46
14	67	7·93	40	70	26	17	21	69	46
15	66	8·64	39	69	25	16	21	68	46
16	65	9·34	38	67	25	16	20	68	46
17	64	10·05	37	66	24	15	20	68	45
18	64	10·75	35	65	23	14	19	67	45
19	63	11·46	34	64	23	14	19	67	45
20	62	12·16	33	63	22	13	18	67	45
21	61	12·83	32	62	21	12	17	66	45
22	60	13·50	30	61	20	11	17	66	44
23	59	14·17	29	59	19	10	16	65	44
24	58	14·84	28	58	18	09	15	64	43
25	57	15·50	27	57	17	08	14	64	43
26	56	16·17	25	56	16	06	14	63	43
27	56	16·84	24	54	16	05	13	62	42
28	55	17·51	23	53	15	04	12	62	42
29	54	18·18	21	52	14	03	12	61	41
30	53	18·83	20	51	13	02	11	60	41
31	52	19·49	19	50	12	01	10	59	40
Feb. 1	55	20·08	17	47	09	2·98	09	58	38
2	54	20·68	16	46	08	97	08	57	37
3	52	21·30	14	44	07	95	07	55	36
4	51	21·92	13	43	05	94	06	54	35
5	50	22·54	11	42	04	92	05	53	34
6	48	23·15	10	40	03	91	04	52	34
7	47	23·77	09	39	02	89	03	51	33
8	46	24·39	07	38	01	88	02	49	32
9	45	25·00	06	37	1·99	86	00	48	31
10	45	25·55	05	36	98	84	1·98	47	30
11	44	26·10	03	34	96	82	97	45	29
12	44	26·64	02	33	95	80	95	44	27
13	44	27·19	01	32	93	78	94	43	26
14	43	27·73	1·00	30	92	76	92	41	25
15	43	28·28	0·98	29	91	74	91	40	24
16	42	28·83	97	27	90	72	89	38	23
17	42	29·37	96	26	88	70	87	37	21
18	41	29·91	94	25	86	68	85	36	20
19	41	30·46	93	23	85	66	84	34	19
20	41	30·91	92	22	83	64	83	33	18
21	40	31·36	91	20	82	62	81	31	16
22	40	31·81	90	19	80	60	80	30	15
23	40	32·26	89	17	78	58	78	28	14
24	40	32·71	88	16	77	56	77	27	13
25	39	33·16	87	15	75	55	75	25	11
26	39	33·61	86	13	74	53	74	24	10
27	39	34·06	85	12	72	51	72	22	09
28	39	34·51	84	11	71	49	71	21	07
29	38	34·96	83	09	70	47	69	19	06

Mean AR 1834.	Sirius h. m. s. 6 37 28·49	Castor h. m. s. 7 23 21·46	Procyon h. m. s. 7 30 5·92	Pollux h. m. s. 7 34 32·16	α Hydræ h. m. s. 9 18 56·44	Regulus h. m. s. 9 58 59·57	β Leonis h. m. s. 11 40 4·75	β Virginis h. m. s. 11 41 31·66	Spica Virg. h. m. s. 13 15 56·07
Jan. 1	+ 2·32″	+ 2·91″	+ 2·41″	+ 2·80″	+ 2·04″	+ 2·03″	+ 1·50″	+ 1·41″	+ 0·84″
2	33	93	42	82	06	06	53	44	87
3	33	94	44	83	08	09	56	48	91
4	34	96	45	85	11	11	60	51	94
5	35	97	47	86	13	14	63	55	98
6	35	99	48	88	15	17	66	58	1·01
7	36	3·01	50	90	17	20	69	62	04
8	37	02	52	91	19	23	73	65	08
9	37	04	53	93	22	25	76	68	12
10	38	06	55	95	24	28	79	71	15
11	38	07	56	96	26	30	82	74	18
12	38	08	57	97	28	33	85	77	22
13	39	09	57	98	30	35	88	79	25
14	39	10	58	99	32	37	91	82	28
15	39	11	59	3·00	34	40	94	85	32
16	39	11	59	01	36	42	96	87	35
17	39	12	60	02	37	44	99	90	38
18	40	13	61	03	39	46	2·02	93	42
19	40	14	62	04	41	49	05	96	45
20	40	15	63	05	43	51	08	99	48
21	40	15	63	06	44	53	11	2·02	51
22	40	16	64	06	46	55	13	04	54
23	40	16	64	07	47	57	16	07	58
24	39	17	65	07	49	59	18	10	61
25	39	17	65	08	50	61	20	12	64
26	39	18	66	09	51	63	23	15	67
27	38	18	66	09	53	64	25	18	70
28	38	19	67	09	54	66	28	20	74
29	38	19	67	10	55	68	31	23	77
30	38	20	68	11	57	70	34	26	80
31	37	20	68	11	58	71	36	28	83
Feb. 1	35	20	67	11	58	72	39	28	89
2	34	20	67	11	59	73	41	30	91
3	33	20	67	11	60	75	44	33	94
4	33	19	67	11	61	76	46	35	96
5	32	19	67	11	62	77	49	37	2·00
6	31	19	67	11	63	79	51	40	02
7	31	19	66	11	63	80	54	42	04
8	30	18	66	11	64	81	56	44	07
9	29	18	66	11	65	83	57	47	09
10	28	17	65	10	65	84	59	49	11
11	27	17	65	10	66	85	61	51	14
12	26	16	64	09	66	86	63	53	16
13	25	15	63	09	67	87	65	55	19
14	24	15	63	08	67	88	67	57	21
15	23	14	62	08	67	88	68	59	24
16	22	13	61	07	68	89	70	60	26
17	21	13	61	06	68	90	72	62	29
18	20	12	60	06	68	91	74	64	31
19	19	11	59	05	69	92	76	66	34
20	18	10	58	04	69	92	77	67	36
21	16	09	57	03	69	93	78	69	38
22	15	08	56	02	69	93	80	70	40
23	13	07	56	01	69	94	81	71	42
24	12	06	55	00	69	94	82	72	44
25	11	05	54	00	69	94	83	73	46
26	09	04	53	2·99	69	95	84	75	49
27	07	03	52	98	69	95	85	76	51
28	06	02	52	97	69	96	87	77	53
29	05	01	51	96	69	96	88	79	55

	Arcturus	2 Libræ*	α Cor. Bor.	α Serpent.	Antares	α Herculis	α Ophiuchi	α Lyræ	γ Aquilæ
Mean AR. [illegible]	h. m. s. 14 7 [illegible]	h. m. s. 14 41 9·[illegible]	h. m. s. 15 [illegible] 14·[illegible]	h. m. s. 15 35 [illegible]·47	h. m. s. 16 18 27·91	h. m. s. 17 6 37·72	h. m. s. 17 26 46·24	h. m. s. 18 [illegible]	h. m. s. 19 [illegible]
Jan. 1	+ 0·55″	+ 0·48″	− 0·04″	+ 0·11″	+ 0·03″	− 0·29″	− 0·31″	− 1·01″	− 0·33″
2	58	45	01	14	06	27	29	00	32
3	61	49	+ 02	17	09	25	27	0·99	31
4	64	52	05	20	12	22	25	98	31
5	67	55	08	23	15	20	23	97	30
6	70	59	11	25	18	18	21	96	29
7	73	62	15	28	21	16	19	94	28
8	76	65	18	31	24	14	17	93	28
9	79	69	21	34	27	11	15	92	27
10	82	72	24	37	30	09	13	91	26
11	85	75	27	40	33	07	11	89	25
12	89	78	30	43	36	04	09	88	24
13	92	82	33	46	39	02	06	86	23
14	95	85	36	49	42	+ 01	04	85	22
15	99	88	39	52	45	03	02	83	21
16	1·02	91	43	55	48	05	00	82	20
17	06	94	46	58	52	08	+ 02	80	18
18	09	98	49	61	55	10	05	79	17
19	12	1·01	52	64	58	12	07	77	16
20	15	04	55	67	61	15	09	75	15
21	18	07	58	70	64	18	11	73	14
22	21	11	61	73	68	20	14	71	12
23	24	14	64	76	71	23	16	69	11
24	27	18	68	79	75	26	19	67	09
25	30	21	71	82	78	28	21	65	08
26	34	25	74	85	82	31	24	62	06
27	37	28	77	89	85	33	26	60	05
28	40	32	81	92	88	36	29	58	03
29	43	35	84	95	92	30	31	56	02
30	46	39	87	98	95	42	34	54	00
31	49	42	90	1·01	98	45	37	52	+ 02
Feb. 1	52	44	94	04	1·02	48	40	49	03
2	55	47	97	07	05	51	43	47	05
3	58	51	1·01	10	09	54	46	44	07
4	61	54	04	13	12	56	48	42	08
5	64	58	07	16	16	59	51	39	10
6	67	61	10	19	19	62	54	36	12
7	71	64	14	23	23	64	56	34	13
8	74	68	17	26	26	67	59	31	15
9	77	71	20	29	29	70	62	29	17
10	80	74	23	32	32	73	65	26	19
11	83	77	26	35	36	76	68	24	21
12	86	80	29	38	39	79	70	21	23
13	89	83	32	41	43	82	73	18	25
14	92	86	35	44	46	85	76	16	27
15	93	89	39	47	50	88	78	13	28
16	97	93	42	50	53	90	81	11	30
17	00	96	45	53	57	93	84	08	32
18	03	99	48	56	61	96	87	05	34
19	06	2·02	51	59	64	99	90	02	36
20	08	05	54	62	67	1·02	93	+ 01	38
21	2·11	07	56	64	70	04	95	03	40
22	13	10	59	67	74	07	98	06	42
23	15	12	62	69	77	09	1·00	09	45
24	18	15	64	72	80	12	03	11	47
25	20	17	67	74	83	14	06	14	49
26	22	20	70	77	86	17	08	17	51
27	24	22	73	79	89	20	11	19	53
28	26	25	75	82	93	22	13	22	56
29	29	28	78	85	96	25	16	25	58

* Mean AR. of 1 α Libræ 14h 40′ 58·21″.

Mean AR. 1824.	α Aquilæ h. m. s. 19 42 11·88	β Aquilæ h. m. s. 19 46 40·23	2* α Capric. h. m. s. 20 8 17·02	α Cygni h. m. s. 20 35 28·21	α Aquarii h. m. s. 21 56 44·67	Fomalhaut h. m. s. 22 47 54·84	α Pegasi h. m. s. 22 56 4·17	α Androm. h. m. s. 23 59 19·69
Jan. 1	− 0·30″	− 0·27″	− 0·09″	− 1·03″	+ 0·22″	+ 0·55″	+ 0·39″	+ 0·70″
2	29	26	08	04	21	54	38	68
3	29	26	08	04	21	53	37	67
4	28	25	07	05	20	52	36	65
5	28	24	06	05	20	51	35	64
6	27	24	06	06	19	50	34	62
7	27	23	05	06	19	49	34	61
8	26	23	05	07	18	48	33	59
9	26	22	04	07	18	47	32	58
10	25	21	03	08	17	46	31	57
11	24	20	02	08	17	45	30	56
12	23	19	01	08	17	44	30	55
13	21	18	00	08	17	44	29	54
14	20	17	+ 0·01	07	17	43	29	53
15	19	16	02	07	17	42	28	52
16	18	15	02	07	17	41	28	51
17	17	13	03	07	17	40	27	49
18	16	12	04	06	16	39	27	48
19	14	11	05	06	16	39	26	47
20	13	10	06	06	16	38	25	46
21	12	09	07	06	16	38	25	45
22	10	07	09	05	16	37	24	44
23	09	06	10	05	17	37	24	43
24	07	04	11	04	17	36	23	42
25	06	03	13	04	17	36	23	41
26	05	02	14	03	17	36	22	40
27	03	00	15	03	17	35	22	38
28	02	+ 01	16	02	17	35	21	37
29	01	03	18	02	18	34	21	36
30	+ 01	04	19	02	18	34	20	35
31	03	06	20	01	18	34	20	34
Feb. 1	04	06	22	00	19	34	20	33
2	06	08	24	0·99	19	34	20	32
3	08	10	25	98	19	34	20	32
4	10	11	27	97	20	33	19	31
5	11	13	28	95	20	33	19	31
6	13	15	30	94	20	33	19	30
7	14	16	32	93	20	33	19	30
8	16	18	34	92	21	32	18	29
9	18	20	35	90	21	32	18	28
10	20	22	37	88	22	32	18	27
11	22	24	39	87	23	32	18	27
12	24	25	41	85	24	32	18	26
13	26	28	43	84	25	33	18	26
14	28	30	45	82	26	33	18	25
15	30	32	47	81	27	33	18	25
16	33	34	48	79	27	33	19	24
17	35	35	50	77	28	33	19	23
18	37	37	52	75	29	34	19	23
19	39	39	54	74	30	34	19	22
20	41	41	56	72	31	34	19	22
21	43	43	58	71	32	35	20	21
22	45	45	60	69	33	35	20	21
23	47	47	62	67	34	36	21	21
24	49	49	64	66	35	36	21	21
25	50	51	66	64	36	37	21	20
26	52	53	67	62	36	37	22	20
27	54	55	69	60	37	38	22	20
28	56	58	71	59	38	38	23	19
29	58	60	73	57	39	39	23	19

* Mean AR of 1 α Capricor. 20h 7′ 53·23″.

	γ Pegasi	Polaris	α Arietis	α Ceti	Aldebaran	Capella	Rigel	β Tauri	α Orionis
Mean AR 1824.	h. m. s. 0 4 11·17	h. m. s. 0 58 2·66	h. m. s. 1 57 16·42	h. m. s. 2 53 5·44	h. m. s. 4 25 50·01	h. m. s. 5 3 42·21	h. m. s. 5 6 5·11	h. m. s. 5 15 10·52	h. m. s. 5 45 38·93
March 1	+ 0·38″	−35·41″	+ 0·82″	+ 1.08″	+ 1·69″	+ 2·45″	+ 1·68″	+ 2·1″	+ 2·05″
2	38	35·74	81	07	67	43	66	16	03
3	38	36·07	80	05	66	41	64	14	01
4	38	36·40	79	04	64	39	63	13	1·99
5	38	36·73	78	02	63	36	61	11	98
6	38	37·06	77	01	61	34	59	09	97
7	38	37·39	76	0·99	60	32	57	07	95
8	38	37·72	75	98	58	30	55	06	94
9	38	38·05	74	97	55	28	54	04	92
10	38	38·38	73	96	54	25	52	02	91
11	38	38·71	72	95	53	23	50	00	89
12	39	38·92	71	94	52	21	48	1·98	87
13	39	39·12	71	93	50	18	47	96	86
14	40	39·33	70	92	49	16	45	95	84
15	40	39·53	70	91	47	14	43	93	83
16	41	39·74	69	90	46	12	41	91	81
17	41	39·95	69	89	44	09	40	89	79
18	42	40·15	68	88	43	07	38	88	78
19	42	40·36	67	87	41	05	36	86	76
20	43	40·56	67	86	40	02	35	84	74
21	43	40·77	66	85	38	2·00	33	82	72
22	44	40·85	66	84	37	1·98	31	80	70
23	45	40·93	65	83	35	96	30	79	69
24	45	41·01	65	83	34	94	28	77	67
25	46	41·09	64	82	33	92	27	75	66
26	47	41·17	64	81	31	90	25	74	64
27	48	41·25	64	80	30	88	24	72	63
28	49	41·33	63	80	28	86	22	70	61
29	50	41·41	63	79	27	83	21	68	60
30	51	41·49	62	78	25	81	19	67	58
31	51	41·56	62	77	24	79	17	65	57

	Sirius	Castor	Procyon	Pollux	α Hydræ	Regulus	β Leonis	β Virginis	Spica Virg.
Mean AR 1824.	h. m. s. 6 37 28·49	h. m. s. 7 23 21·46	h. m. s. 7 30 5·32	h. m. s. 7 34 32·18	h. m. s. 9 18 56·44	h. m. s. 9 58 59·57	h. m. s. 11 40 4·73	h. m. s. 11 41 31·96	h. m. s. 13 15 56·07
March 1	+ 2·04″	+ 3·00″	+ 2·50″	+ 2·95″	+ 2·69″	+ 2·96″	+ 2·89″	+ 2·80″	+ 2·57″
2	02	2·99	49	94	69	96	90	81	59
3	01	97	48	92	68	96	91	82	61
4	1·99	96	47	91	68	96	92	83	63
5	97	94	46	90	67	96	93	84	64
6	96	93	45	89	67	97	94	85	66
7	94	91	43	87	66	97	95	86	68
8	92	89	42	86	66	97	96	87	70
9	91	88	41	85	65	97	97	88	72
10	89	87	40	83	65	97	98	89	74
11	87	86	39	82	64	97	99	90	76
12	85	84	38	80	63	96	3·00	91	78
13	83	83	36	79	62	96	00	91	79
14	82	81	35	77	62	95	01	92	81
15	80	80	34	76	61	95	01	93	82
16	78	78	33	74	60	94	02	93	84
17	76	77	31	72	59	94	02	94	85
18	75	75	30	71	58	93	03	95	87
19	73	73	29	70	58	93	03	95	88
20	71	71	27	68	57	92	04	96	90
21	69	69	26	66	56	92	05	97	92
22	67	67	24	64	55	91	05	97	93
23	65	66	23	63	54	91	05	97	94
24	63	64	21	61	53	90	05	97	95
25	61	62	20	60	52	89	05	98	96
26	59	61	18	58	51	89	05	98	98
27	57	59	16	56	50	88	06	98	99
28	55	58	15	55	49	87	06	98	3·00
29	53	56	13	53	48	87	06	99	01

	Arcturus	2 α Libræ	α Cor. Bor.	α Serpent	Antares	α Herculis	α Ophiuchi	α Lyræ	γ Aquilæ
Mean AR 1824.	h. m. s. 14 7 38·38	h. m. s. 14 41 9·68	h. m. s. 15 27 14·45	h. m. s. 15 35 36·47	h. m. s. 16 18 37·91	h. m. s. 17 6 37·72	h. m. s. 17 26 46·24	h. m. s. 18 30 58·99	h. m. s. 19 37 53·68
March 1	+ 2·31″	+ 2·31″	+ 1·81″	+ 1·88″	+ 1·99″	+ 1·28″	+ 1·19″	+ 0·28″	+ 0·60″
2	33	34	84	91	2·02	31	22	31	63
3	36	36	87	93	05	33	25	34	65
4	38	39	90	96	09	36	28	37	68
5	40	41	93	99	12	38	31	41	70
6	43	44	96	2·01	16	41	34	44	73
7	45	46	98	04	19	43	37	47	75
8	47	49	2·01	07	22	46	40	50	78
9	49	52	04	10	26	49	43	54	81
10	52	55	07	12	29	52	46	57	84
11	54	57	10	15	32	54	49	60	86
12	56	59	12	18	35	57	52	63	88
13	58	62	15	20	38	61	55	67	91
14	60	64	17	23	41	64	58	70	93
15	62	66	20	25	44	67	61	74	96
16	64	68	22	28	47	70	64	77	98
17	66	71	25	30	50	74	67	80	1·01
18	67	73	27	33	54	77	69	83	03
19	69	75	30	36	57	80	72	87	06
20	71	78	32	39	60	84	75	91	08
21	73	80	35	41	63	87	78	94	11
22	75	82	37	43	66	90	81	97	14
23	76	84	40	46	69	93	84	1·01	16
24	78	86	42	48	72	95	87	04	19
25	80	88	44	50	75	98	90	07	22
26	81	90	46	52	78	2·00	93	10	24
27	83	92	49	55	81	03	95	13	27
28	85	95	51	58	84	06	98	17	29
29	86	97	53	60	87	08	2·01	20	32
30	88	99	56	62	90	11	04	24	35
31	90	3·01	58	64	93	14	07	27	38

	α Aquilæ	β Aquilæ	2 α Capricor	α Cygni	α Aquarii	Fomalhaut	α Pegasi	α Androm.
Mean AR 1824.	h. m. s. 19 42 11·88	h. m. s. 19 46 40·28	h. m. s. 20 8 17·02	h. m. s. 20 35 26·21	h. m. s. 21 56 44·67	h. m. s. 22 47 54·34	h. m. s. 22 56 0·17	h. m. s. 23 59 18·57
March 1	+ 0·60″	+ 0·62″	+ 0·75″	− 0·55″	+ 0·40″	+ 0·39″	+ 0·23″	+ 0·19″
2	62	64	77	53	41	40	24	19
3	65	67	80	50	43	41	25	19
4	67	69	82	48	44	42	25	19
5	70	72	85	45	45	43	26	19
6	72	74	87	43	46	44	27	19
7	75	76	90	40	48	45	28	19
8	77	78	92	38	49	46	29	19
9	80	81	95	35	50	47	29	19
10	82	83	97	32	52	48	30	19
11	85	86	1·00	30	53	49	31	19
12	88	88	02	27	55	50	32	19
13	90	91	05	24	56	52	33	20
14	93	93	07	22	58	53	34	20
15	95	96	10	19	60	54	35	21
16	98	98	12	16	61	56	36	21
17	1·00	1·01	15	13	63	57	37	22
18	03	03	17	11	64	58	39	22
19	05	06	20	08	66	60	40	23
20	08	08	22	05	68	61	41	23
21	11	11	25	02	70	63	42	24
22	14	14	28	+ 01	72	65	43	25
23	17	17	31	04	74	66	45	26
24	19	19	33	08	76	68	46	27
25	22	22	36	11	78	69	48	28
26	25	25	39	14	80	71	49	29
27	28	28	42	17	82	73	51	30
28	31	31	44	20	84	75	52	31
29	33	33	47	23	86	76	54	32
30	36	36	50	27	88	78	55	33

Article IX.

Description of a New Thermoelectric Instrument. By the Rev. J. Cumming, Professor of Chemistry in the University of Cambridge.

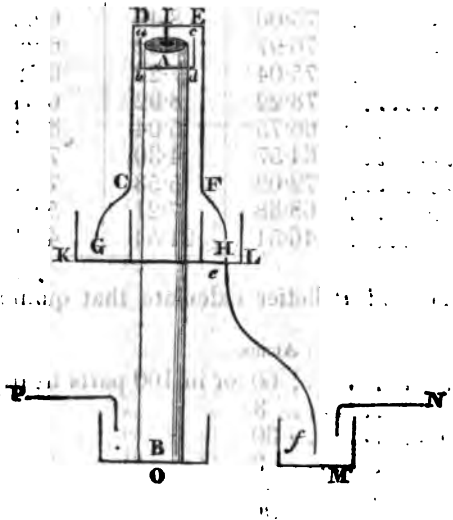
(To the Editor of the *Annals of Philosophy.*)

MY DEAR SIR, *Cambridge, Dec. 20, 1822.*

Whatever contributes to confirm the close analogy which subsists between the electricity excited by heat and that by galvanic action, will, I conceive, be acceptable to those who take an interest in this subject. For this purpose, I have constructed an instrument, the description of which you will oblige me by inserting in the next number of the *Annals of Philosophy.* It exhibits the rotation of a wire round a magnet, and its delicacy is such that when excited by the thermoelectricity of a silver and platina wire, each of 1-22d inch diameter, it revolves between 30 and 40 times in a minute; if, instead of these wires, a pair of galvanic plates of half an inch in diameter be used, the rotation is rather more rapid.

I am, my dear Sir, very truly yours,

J. Cumming.



A B, a cylindrical magnet.

a b c d, a glass tube containing mercury, cemented on the top of the magnet.

C D E F, a brass wire poised by a needle point passing through I, and resting upon an agate cemented upon the magnet.

C G, F H, platina points soldered to the wire C D E F.

K L, a cylindrical piece of wood, having a perforation to admit the magnet, and a circular groove containing mercury, in which the points G and H revolve.

e f, a copper wire passing through the bottom of K L, and communicating with the mercury in the groove.

M O, cups filled with mercury.

P N, wires passing from the positive and negative ends of the exciting apparatus.

The current from the positive pole P ascends through the magnet to I, descends down D G, E H, into the mercury in the circular groove, and from thence through the copper wire *e f*, into the cup M, connected with the negative pole by the wire N.

Article X.

On Organic Salifiable Bases. By MM. Dumas and Pelletier.*

The following is the analysis of these compounds given by these chemists:

	Carbon.	Azote.	Hydrogen.	Oxygen.
Quinina	75·00	8·45	6·66	10·43
Cinchonia	76·97	9·02	6·22	7·79
Brucia	75·04	7·22	6·52	11·21
Strychnia	78·22	8·92	6·54	6·38
Veratria	66·75	5·04	8·54	19·60
Emetin	64·57	4·30	7·77	22·95
Morphia	72·02	5·53	7·01	14·84
Narcotin	68·88	7·21	5·91	18·00
Cafein	46·51	21·54	4·81	27·14

MM. Dumas and Pelletier calculate that quinina is constituted of

	Atoms.	or in 100 parts nearly
Carbon	60	75·38
Azote	3	8·72
Hydrogen	30	6·15
Oxygen	3	9·85
	96	100·00

If, however, we calculate the weights of the atoms as given by Dr. Thomson, Dr. Henry, and Mr. Brande, it will be found that the number of atoms which enter into the constitution of this, and probably the other substances, are much fewer, and the calculated result is rather nearer that obtained by experiment.

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* From the Annales de Chimie et de Physique, vol. xxiv. p. 191.

Carbon..	20 atoms	6 × 20 =	120	or in 100 parts	75·00
Azote....	1	=	14		8·75
Hydrogen.	10	=	10		6·25
Oxygen..	2	8 × 2 =	16		10·00
	33		160		100·00

In concluding, the authors observe, that the results of the analyses of the substances in question are equivalent to

	Carbonic acid.	Azote.
Quinina	100	5·1
Cinchonia	100	5·0
Strychnia	100	4·9
Narcotin	100	4·5
Brucia	100	5·0
Morphia	100	3·2
Veratria	100	3·2
Emetin	100	3·1
Cafein	100	20·0

With respect to the last mentioned substance, MM. Dumas and Pelletier observe, that no particular memoir has as yet appeared upon it. It was discovered in 1821 by M. Robiquet, in his researches to discover quinina in coffee. The authors of this paper obtained the same principle about the same time, but the priority is due to M. Robiquet. The properties of cafein are stated to be, that it is white, crystalline, volatile, and but slightly soluble.

The method of analysis adopted was that suggested by Gay-Lussac; the peroxide of copper employed was prepared by calcining the nitrate at a dull red heat; it was then carefully washed, and again heated at the same temperature to expel the moisture; and before use it was moderately heated in a platina crucible, and weighed while still warm.

MM. Dumas and Pelletier adopt the specific gravity of gases as determined by Dulong and Berzelius, and their calculations are founded on the weights of atoms given by the same chemists.

ALBITE.

Fig. 1.

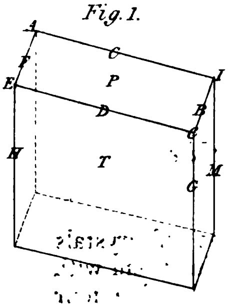


Fig. 2.

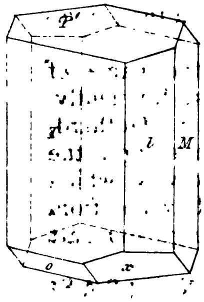


Fig. 3.

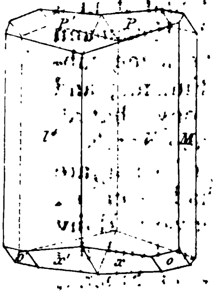
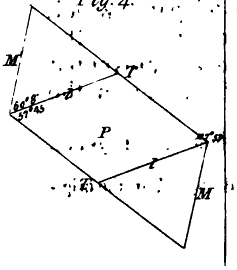


Fig. 4.



ANORTHITE.

Fig. 8.

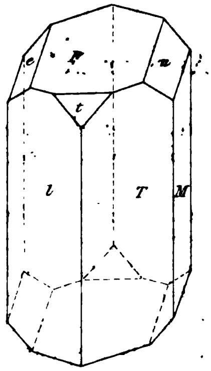


Fig. 6.

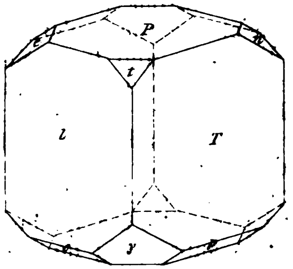


Fig. 7.

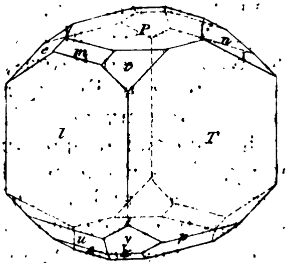


Fig. 5.

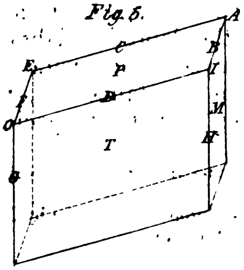
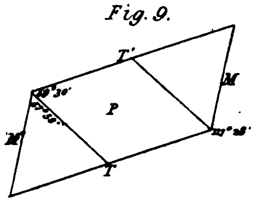
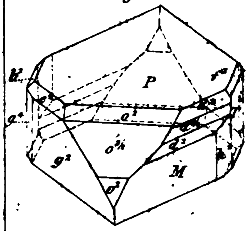


Fig. 9.



CLEAVELANDITE.

Fig 10.



FORSTERITE.

Fig. 11.

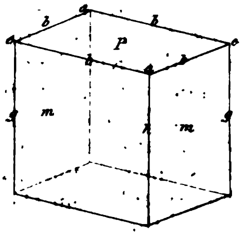
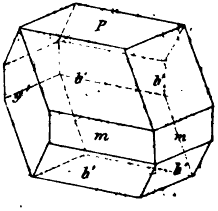


Fig. 12.



Engraved for the Annals of Philosophy, for Baldwin, Cradock & Joy, Jan 1824

ARTICLE XI.

On Felspar, Albite, Labrador, and Anorthite.

By M. Gustavus Rose.*

SOME differences I had found in the angles of crystals described hitherto as felspar, induced me to examine them with greater accuracy. From my observations, it results that four different species, which differ as much in their form as in their chemical composition, had been united under the common name of felspar; it is true that there is a great analogy in their crystalline forms.

Among these species, that which will retain the name of felspar, $K S^3 + 3 A S^3$, is the one met with most frequently. Under that name must be classed the adularia from St. Gothard, vitreous felspar from Vesuvius and from Siebengebirge, the amazon-stone, the felspar from Friedrichwarn, in Norway, which had been taken for labrador, the felspar from Baveno, from Carlsbad, from Fichtelgebirge, and in general the greater part of what Werner has called common felspar.

The second species called albite (cleavelandite†) $N S^3 + 3 A S^3$ is not so common as felspar. We are indebted to M. Eggerts for the first notice of this substance; he examined a radiated variety of it from Finbo and from Broddbo, near Fahlun. Since, MM. Hausmann and Stromeyer have also found it in a rock from Chesterfield in North America, and M. Hausmann named it Kieselspath. M. Nordenskiold found the same substance in a granite from Kimito, near Pargas, in Finland; and lastly, M. Ficinus in a granite from Penig, in Saxony; but all these varieties were not regularly crystallized. The crystals of the same substance which I have had an opportunity of seeing, are the crystals from Dauphiny, which Romé de L'Isle had described under the name of *schorls blancs,* and which afterwards Haüy took for felspar; the crystals from Salzbourg and the Tyrol, described as adularia; the crystals from Kerabinsk in Siberia, from Arendal in Norway, from Prudelberg near Stirschberg in Silesia; as well as many other crystals from different localities.

The third species is the labrador (labrador-felspar) which Klaproth had already analyzed and separated from felspar; the external characters of this substance had, however, prevented mineralogists from making a distinct species of it. From the analysis of Klaproth, M. Berzelius has found that the formula was $N S^3 + 3 C S^3 + 12 A S$.

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* Translated (with some omissions) from the Annales de Chimie et de Physique, tome xxiv. p. 5.

† This name (cleavelandite) was proposed for albite by Mr. Brooke, but the original term has been preserved in this translation.

The fourth species is the scarcest of all; I have only met with it in small groups of crystals in blocks of carbonate of lime, which are found near Vesuvius. I have found that their chemical formula was $M S + 2 C S + 8 A S$, and I have given them the name of *anorthite*.

I shall now describe the principal properties of these four species. In the description of the crystals I have only given the primitive form, the signs of the secondary planes, and the principal angles. I have thought it useless to describe more minutely the secondary crystals.* The figures I have given, especially when compared with their signs, are perfectly sufficient to form an exact idea of the relative situation of these planes, and of the parallelism of the edges. The signs of the secondary planes are given according to the method of Haüy, and I have calculated them from the angles of the primitive form, which I have measured with as much exactness as possible, by means of spherical trigonometry, and by the parallelisms of the edges. But the primitive forms of these species being doubly oblique prisms, the theory of which is not yet perfectly known, their determination depends on five measurements, while the determination of oblique rhombic prisms depends only on two: it is for that reason that I can only consider the angles I have given as approximations not very far from the truth.

The specific gravity has been determined with care. When I had only small crystals to examine, I weighed some of them in a small flask of glass, the weight of which both in water and in air was subtracted from the weight of the flask containing the small crystals, and weighed under the same circumstances. I have given the temperature of the water I used in my experiments. I have not reduced my results to the same temperature, because they would be but very little altered by that reduction.

The hardness of all the species described is less than that of quartz, and differs but little from that of felspar. Albite has in general appeared to me to be the hardest, and labrador the softest.

First Species.—Felspar.

The system of crystallization is, according to M. Weiss, *bino-singulaire*. The primitive form is an oblique rhombic prism, in which the ratio of the three dimensions which are perpendicular to each other, and equal to the diagonals of the section perpendicular to the lateral edges, and to the length of one of these edges is $= \sqrt{13} : \sqrt{3 \cdot 13} : \sqrt{3}$.

The chemical formula is, according to Berzelius, $K S^3 +$

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* The figures of crystals are not all given in the translation.—*Edit.*

$3 A S^3$. If we calculate from this formula the proportion of the constituent parts, we find that 100 parts of felspar contain

Silex	65·94
Alumina	17·75
Potash	16·31

Observations.—Although felspar is common, yet it is rarely met with in such perfectly brilliant crystals as are necessary for measurement by the reflecting goniometer. The collection of minerals in the University of Berlin, which is extremely rich in crystals of felspar, does not contain a specimen the crystals of which could have been measured by that instrument. The best for that purpose with which I am acquainted are the crystals of glassy felspar from Vesuvius, and I have measured the angles of some which differ a little from those given by M. Weiss. I have found for instance the obtuse incidence of the lateral planes of the primitive to be 119° 18′, and that of the base of the primitive upon one of the lateral planes 112° 14½′*. These measurements, however, I did not consider as sufficiently exact to ground my calculations upon.

I was rather surprised by what I found to be the specific gravity of the felspar of Baveno. I had weighed it several times, and I had chosen not only the hemitrope crystals which are so frequently met there, but also simple crystals which are perfectly pure, and did not appear to contain any foreign substance. The results I obtained were always the same, and I was induced to think that the composition of the crystals of Baveno differed from that of felspar, and that since the crystallisation was perfectly the same in both, some isomorphous principle was replaced by another. I, therefore, analyzed a crystal from Baveno. In fusing it with carbonate of potash, and in treating it in the usual manner, I found the proportion between the silex and the alumina exactly the same as that which exists in common felspar; so that though I had not separated the potash, I thought I had no reason to suppose the composition different from other felspars.

Second Species.—Albite.

The primitive form of albite is a doubly oblique prism (Plate XXV), figs. 1, 2. The planes M and T of which are inclined at an angle of 117° 53′; the planes M P form angles of 93° 36′ and 86° 24′; the planes T and P angles of 115° 5′ and 64° 55′. The section perpendicular to the planes M and T is an oblique-angled parallelogram, fig. 2, the obtuse angle of which is divided by a plane *l* produced by a decrement of two rows along the edge G, into two angles of 60° 8′ and 57° 46′, the first of which has one of its sides situated in the plane M, and the second, one of its sides in the plane T. The section perpendicular to the planes M and P is an oblique-angle parallelogram, the obtuse angle of which is divided by the plane *n*, produced by a decrement by one row on the edge B, into two angles of 46° 5′ and 47° 31′; the first of which corresponds to the edge of the parallelogram situated in the plane P; the other to the edge of the parallelogram situated in the plane M.

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* Mr. W. Phillips gives for the same angles 119° [illegible] and 11[illegible]

The planes I have observed are

$$P\ M\ T\ \underset{l}{G}\ \underset{z}{G^{4}}\ \underset{f}{{}^{2}H}\ \underset{y}{\overset{1}{A}}\ \underset{x}{\overset{2}{A}}\ \underset{n}{\overset{\frac{1}{2}}{B}}\ \underset{o}{\overset{2}{C}}\ \underset{g}{\overset{4}{C}}$$ (see figs. 3, 4).

*Incidences.**

T on M′	117°	53′
T on *l*	122	15*
M on *l*	119	52*
M on *z*	149	12
l on *z*	150	40
M′ on *f*	148	30
T on *f*	149	23
P on T	115	5*
P on *l*	110	51*
P on *o*′	122	23*
M on *o*′	112	11
P on *g*	150	5
M on *g*	100	52
P on M	86	24
P on *n*	133	55
P on *y*′	97	37
T on *y*′	134	32
T on *x*′	110	29
P on *x*′	127	23

Plane Angles of the Primitive Form.

Those of plane	P	119°	12′ and	60°	8′
	M	116	35	63	25
	T	99	45	80	15

The crystals of albite are frequently or almost always met under the form of hemitropes.† These hemitropes are formed when two crystals are so joined to each other that the upper plane of the one is applied upon the inferior plane P′ of the other, in the manner exhibited by fig. 3. The two crystals have generally the same size; however all the differences which are known to occur in the hemitropes are also met with in this substance; frequently one of the crystals is only visible by a narrow line on the plane P of the other. A third crystal is often applied on the second; and a fourth upon the third, &c. The hemitropes attached to the matrix present always the same end upwards, and that corresponds to the upper part of fig. 3.

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* I have marked * the angles from which the others are calculated.

† I found, however, afterwards, that the crystals of St. Gothard, the prisms of which are so short that the planes of one of the summits meet those of the other, are very likely albite: they are met commonly in simple crystals, and seldom in hemitropes. Their planes were not sufficiently brilliant to be measured; but it is likely that they were albite, since, when digested in hydrochloric acid, they were not decomposed.

This substance can be cleaved parallel to every plane of the primitive; the cleavage parallel to P is the most brilliant. The colour of the crystals is white or reddish-white; the crystals are translucent or transparent, either wholly or in part as in those of Kerabinsk.

The specific gravity will be found in the following table:

	Locality.	Weight gram.	Sp. gr.	Tem. water.
Hemitrope crystals	Kerabinsk	4·808	2·608	20° R.
Hemitrope crystals	Kerabinsk	12·711	2·6175	21½
Id. reddish-white	Arendal	3·692	2·619 2·614	17 17½

The specific gravity has been found before by

Eggertz, that of Finbo	2·612
Eggertz, that of Broddbo	2·619
Nordenskiold, that of the red albite from Kimito	2·609
Ficinus, that of the albite of Penig	2·50

The result of an analysis of crystallized albite from Arendal, decomposed by means of carbonate of potash, is

Silica	68·46	which contains oxygen	34·43	12
Alumina	19·30		9·01	3
Lime	0·68			
Oxide of iron	0·28			
Magnesia				
Loss	11·27	taken as soda	2·88	1

Another analysis in which I had precipitated the alumina with carbonate of potash, gave the following result:

Silex	68·60
Alumina, with a little oxide of iron	19·25

An analysis with carbonate of barytes, gave

Silex	68·84
Alumina, with a little oxide of iron and lime.	20·53
Soda	9·12
	98·49

If the composition of albite is calculated from the formula $N S^3 + 3 A S^3$, the following proportion of the constituent parts is found

Silex	69·78
Alumina.	18·79
Soda	11·43

Crystallized albite is found at Arendal in Norway, where it is almost always accompanied with epidote, according to what I have seen at the place itself, as well as in private collections. It is found also in the Schmirnerthal, in the Tyrol, with carbonate of lime in veins of carbonate of lime; at Rohrberg, near Zell, in veins with quartz, or in gneiss very rich in quartz, accompanied by rock crystal and carbonate of iron: it is found in the same circumstances at Gastein, in the country of Salzbourg; at Bareges in the Pyrenees, and at Auris in Dauphiny, in veins with axinite, anatase, adularia, epidote, asbestus, with which the albite is sometimes perfectly mixed. As to the albite of Kerabinsk, in Siberia, the collection of minerals in the University of Berlin, contains only isolated hemitrope crystals, which are of a much larger size than the others. Sometimes the plane M is one inch long, while the other hemitrope crystals are never more than a few lines. At Prudelberg, at Stonsdorf, near Hirschberg, in Silesia, albite is found with felspar in veins of granite; the crystals of felspar are flesh-coloured, and sometimes covered with crystals perfectly white, or of the same colour as those of albite. The crystals of felspar of Baveno are also frequently accompanied by some small whitish crystals, which commonly are not felspar, but albite.*

Observations.—The crystals of albite are easily distinguished by their hemitropes, and the re-entering angles formed by the planes P. If the crystals of felspar were grouped in the same way, the similar planes of the two crystals would be parallel, since in felspar the planes M and P are at right angle to each other, and could never form re-entering angles; the analogous hemitrope crystals of felspar, such as those of Carlsbad, can only be formed as it has been demonstrated by M. Weiss, when two crystals are grouped, either with their right planes M, or with their left planes M. So that the faces P of cleavage are situated on opposite sides in the two crystals, while in albite the planes P of the two crystals are situated on the same side.

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* If the above-mentioned crystals of St. Gothard are albite.

Albite offers, however, sometimes crystals which are grouped in a manner analogous to the hemitrope crystals of felspar. They are joined to each other by their planes M, and consequently have their planes P on different sides; but in this case the two crystals are attached by their other faces to other crystals in the common way; so that the whole is only an hemitrope formed by two different hemitropes which are grouped in the same manner as the two simple crystals which form the hemitrope crystals of felspar of Carlsbad.

Although albite is found massive, it is always radiated, never in laminæ, and that distinguishes it essentially from felspar. It may always be admitted, therefore, that the felspar which is met in this state is not felspar, but albite. The palmed felspar of Johann Georgenstadt in Saxony, distinguished by Werner, is among those of this kind the most known in Germany: however, some doubts may be entertained concerning several specimens of various localities contained in the collection of minerals at Berlin.

Besides the albite of Arendal, I have analyzed that of Salzbourg. Some circumstances have prevented me terminating the analysis of it; however, I have obtained soda, and the same quantity of silex, as in the analysis of the albite of Arendal.

The sulphate I had obtained, and which I had crystallized with a great deal of care, gave me crystals perfectly similar to those of sulphate of soda. When exposed to the atmosphere, they fall to powder, and treated by the solution of platina, by tartaric acid, and by sulphate of alumina, they exhibited the same properties as sulphate of soda. Having mixed a solution of these crystals with a solution of chloride of platinum in alcohol, it remained perfectly limpid, and evaporated to dryness, and left a mass perfectly soluble in alcohol. A solution of these crystals into which I had put tartaric acid, retained its limpidity. In mixing this with sulphate of alumina and alcohol, I obtained regular octohedrons perfectly well crystallized, which I consider as sulphate of alumina and soda; because when opposed to the atmosphere, they were reduced to a fine powder, and are thus sufficiently distinguished of sulphate of alumina and potash, which mixed with alcohol was immediately precipitated in a state of powder.

In analyzing albite with carbonate of barytes, I have found a loss of 2½ per cent. It is undoubtedly soda which suffers this loss; this appears to me so much the more likely, for I obtained silex and alumina in the same proportions as in analyzing albite with carbonate of potash, and the result was the same in calculating the proportion of these two bodies from the same chemical formula. I could not repeat the analysis, because I had used all I had of the substance to determine the nature of the alkali contained in albite, and for the analysis with the carbonate of potash.

Third Species.—Labrador.

This substance is very seldom met in regular crystals. There is only one specimen in the collection of minerals at the University of Berlin; and although it is possible to determine the form of it, which shows great analogy with felspar, the angles cannot be measured. The modifications appear to be the same as those of felspar. It cleaves easily in two directions, in one of which the face obtained by cleavage is perfectly brilliant; the difference between the degree of brilliancy of these two cleavages denotes a difference between labrador and felspar. Moreover those two cleavages are not obviously at right angles to each other. I have found their inclination to be 93¼° and 86¾°. I could not measure more exactly the incidence of these two cleavages on account of the dulness of one of them. There is a third cleavage still more imperfect, and which corresponds with one of anorthite, but not with any of albite.

Thin laminæ of labrador are of a whitish-grey; the fine reflection of light which distinguishes this substance is given by one of the cleavages.

The specific gravity of a fragment of labrador (from Labrador, in America) weighing 10·576 grs. was found, using water at the temperature of 18° R. = 2·7025.

The specific gravity of a fragment weighing 12·068 gr. from the same locality, using water at the temperature of 17¼° R. = 2·695.

According to Brisson, = 2·692.

According to Klaproth, = 2·690.

Specific gravity of the labrador from Ingremanie, according to Klaproth, = 2·750.

One hundred parts of labrador from Labrador, and an equal quantity of labrador from Ingremanie, contain, according to Klaproth,

	Labrador from Labrador.	Labrador from Ingremanie.
Silex	55·75	55·00
Alumina	26·50	24·00
Lime	11·00	10·25
Oxide of iron.	1·25	5·25
Soda	4·00	3·50
Water	0·50	0·50
	99·00	98·50

Berzelius has calculated from these analyses the mineralogical formula

$$N S^3 + 3 C S^3 + 12 A S.$$

Observations.—Labrador and felspar present similar characters with the blowpipe; and for this reason Berzelius was induced to suppose that the mineral analyzed by Klaproth, under the name of labrador, was iridescent parenthine, with which it has great analogy of composition. However, an analysis undertaken by my brother, gave, excepting a greater quantity of alumina, almost the same results as that of Klaproth. This chemist has already demonstrated that the iridescent felspar from Friedrichwarn, in Norway, cannot be ranked in this class; it is also distinguished from it by the incidence of the two faces of cleavage which is equal to 90°. The acids act upon this mineral in a different manner than upon felspar and albite; for concentrated hydrochloric acid, according to Fuchs, entirely decomposes labrador, and has no action upon felspar or albite.

Fourth Species.—Anorthite.

The primitive form of anorthite is a doubly oblique prism, fig. 5, 6, in which the planes M and T are inclined at an angle of 117° 28′; the planes M and P at an angle of 94° 12′, and the planes T and P at an angle of 111° 57′. The section perpendicular to the planes M and T is an oblique angled parallelogram, the obtuse angle of which of 117° 28′ is divided by the plane produced by two rows in breadth on the edge G into two angles, the one of 59° 30′ and the other of 57° 58′; the first of which has one of its sides in the plane T, and the other one of its sides in the plane M. The section perpendicular to the planes M and P is an oblique angled parallelogram, the obtuse angle of which equal to 94° 12′ is divided by a plane produced by a decrement by one row on the edge B of the primitive into two angles, the one of 46° 47′, and the other of 47° 25′; the first of which has one of its sides in the plane P, and the other one of its sides in the plane M. The planes I have observed are:

$$P\ M\ T\ \underset{l}{{}_2G}\ \underset{f}{{}_4G}\ \underset{z}{H^3}\ \underset{e}{\overset{1}{T}}\ \underset{n}{\overset{1}{B}}\ \underset{p}{\overset{2}{C}}\ \underset{y}{\overset{1}{A}}\ \underset{x}{\overset{2}{A}}\ \underset{q}{\overset{3}{A}}\ \underset{t}{\overset{1}{O}}\ \underset{o}{A^{\frac{1}{2}}}\ \underset{u}{\overset{2}{A}}\ \underset{m}{{}_2\overset{1}{O}}\ \underset{v}{{}_4A}\ \underset{w}{E^2}\ \text{(figs. 7, 8, 9).}$$

Incidences.

T on M	117°	28′
T on *l*	120	30
M′ on *l*	122	2
M on *z*	149	1
T on *z*	14	7
M′ on *f*	15	[illegible]
P on *y′*	98	29
P on *x′*	128	27
P on *q′*	145	12
P on *t*	138	46
P on *o′*	121	50
P on *u′*	94	53
P on *m*	134	46
l on *f*	151	28
P on M	85	48*
P on *n*	133	18*

P on [illegible]	137° 22′
P on T	110 57
P on p′	125 38
M on o′	115 20
M on u′	122 45
M′ on m	116 12
P on v′	91 56
M on v′	141 54
P on u′	98 37
M′ on w′	141 22

Plane Angles of the Primitive Form.

Those of plane P	121° 33′	and 58° 27′
M	116 15	63 45
T	106 42	73 18

Anorthite, as well as albite, although not quite so frequently, presents also hemitrope crystals, I have not given drawings of them, because they are formed exactly according to the same laws. This substance can be cleaved parallel to the planes P and M with equal facility. I have not been able to obtain a cleavage parallel to the plane T, and I have chosen it for one of the primitive planes in preference to the plane *l*, because it is generally much more brilliant, The fracture in other directions is conchoidal. The lustre of the cleavages is pearly, and that of the conchoidal fracture vitreous.

Anorthite is found sometimes crystallized in small masses. The crystals are perfectly clear and transparent, but very small.

The specific gravity of several fragments weighing 1·463 gr. by using water at the temperature of 14° R. has been found equal to 2·763.

That of small crystals weighing 0·316 gr. mixed with a small quantity of pyroxene, by using water at 17° R. was found equal to 2·656.

Concentrated hydrochloric acid entirely decomposes anorthite. I have found 100 parts of anorthite, the specimens of which, as well as those of albite, I had obtained through the kindness of Mr. Weiss, from the collection of minerals in the University of Berlin, composed of

Silex	44·49	which contains oxygen	22·38	11
Alumina	34·46	16·096	16·326	8
Oxide of iron	0·74	0·23		
Lime	15·68		4·40	2
Magnesia	5·26		2·04	1

Another analysis in which I had only 0·6 gr. to examine gave, however, similar results, and consequently the mineralogical formula is

$$MS + 2CS + 8AS$$

when one part of 8 A S is replaced by F S. Anorthite has only been found hitherto in masses of carbonate of lime at Mount Somma, near Vesuvius, where it is accompanied only by green translucent pyroxene.

Observations.—The mineralogical formula indicated above, appears to be the result of the analyses: I cannot, however, warrant its exactness, because I could only operate upon very small quantities; the first time with 0·628 gr.; the second time with 1·482 gr.: it is the result of this last analysis I have given. The formula would be analogous to other formulæ already known, if there was 9 A S, instead of 8 A S. Then it would be the same as that of meionite and paranthine, the formula of which is C S + 3 A S, with this difference, however, that one-third of C S in anorthite would be replaced by M S. Anorthite would then be referred to meionite, in the same manner as idocrase is to garnet, or, according to my brother's analysis, pyroxene to wollastonite.

I have provisionally given the name of anorthite to this mineral, derived from *ανορθος*, which signifies without right angles; because its crystalline form is principally distinguished from felspar, in not being at right angles to each other. Haüy, to whom the name of felspar did not seem proper, had suggested for this mineral the name of *orthose*, from two of its cleavages being at right angles to each other.

Article XII.

Observations on the preceding Paper, with an Account of a new Mineral. By M. Levy, MA. of the Academy of Paris.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Dec.* 20, 1823.

Since the notice you inserted in one of the preceding numbers of the *Annals of Philosophy* of the division I had made of the specimens commonly ranked under the name of felspar, into two distinct species, viz. felspar and cleavelandite, I have seen in the last number of the Annales de Chimie a paper by M. Rose, of Berlin, upon the same subject. An abstract of this paper is inserted in the present number of the *Annals*, and contains, in addition to the essential part of what I intended to publish, not only new analyses of both felspar and cleavelandite, and their specific gravities, but also the complete determination of two new species, viz. labrador and anorthite. In consequence of this, I shall limit what I proposed to send you, to a very few observations, which M. Rose's paper does not render useless.

M. Rose has adopted, as appears from a determination of Weiss, an oblique rhombic prism for the primitive of felspar. I had assumed the same form, from the observation of the crystals of that substance I had an opportunity of examining in Mr. Turner's collection, as well as from the very figures given by Haüy, and the measurements given both by him and Mr. W. Phillips. M. Rose has not stated the reasons which induced Weiss to alter the determination of Haüy; and as I believe they are not generally known, since Mr. Brooke and Mr. W. Phillips, in their late publications, have adopted the primitive form of Haüy, I shall briefly explain by what considerations I was led to the same result as Weiss.*

On looking at the figures given by Haüy in the last edition of his treatise on mineralogy, as well as on looking at any crystal of felspar, it will easily be seen that every one of them may be derived from an oblique rhombic prism, the lateral planes of which would be, for instance, the plane he has marked *l*, and the face opposite and parallel to T, and the base the plane P. If the primitive were not such an oblique rhombic prism as I have just described, one would expect to meet with a crystal containing the face T without the face *l*, or the modification *z* without the modification *z'*, or *s* without *s'*, or *n* without *n'*, but the constant simultaneous occurrence of these groups of modifications perfectly symmetrical relative to the planes P, *l*, T, both in their positions and incidences, is certainly decisive. Moreover, in the form I have adopted, if a cleavage be found parallel to M, it must be at right angles to the base P, because M is equally inclined upon *l* and T, or because it is parallel to a plane through the oblique diagonals of the bases. This cleavage, as it is well known, exists in felspar, and is found perpendicular to P. This angle of 90° would again be a very singular occurrence if the primitive were a doubly oblique prism. The only argument in favour of Haüy's determination is, his assertion that there is a cleavage parallel to T, and none parallel to *l*, as should be the case, if *l*, as I have assumed, was one of the lateral primitive planes symmetrical to T. To this may be answered that even the cleavage parallel to T is in most cases very difficult to obtain, that this is not the only example of an oblique rhombic prism, in which one of the lateral planes is more easily obtained by cleavage than the other.† It is the case, for instance, in chromate of lead. Moreover, in some of the flesh-coloured specimens, I have succeeded in obtaining a cleavage parallel to *l*, by detaching first a thin lamina parallel to P. Finally, Haüy mentions the primitive he has adopted as one of the forms offered by nature: this form I have never seen; and I doubt very much its existence, because it could not be derived from an oblique rhombic prism from which all the others are so obviously deduced.

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* See *Annals of Philosophy* for November.

† See Brooke's Familiar Introduction to Crystallography, p. 189.

The form M. Rose has taken for the primitive of cleavelandite differs only in its angles from that I had assumed. He gives for the incidence of T on M 117° 53′. I have constantly found it upon brilliant cleavage planes 119° 30′, or between 119° 30′ and 120°, which makes a difference of 2° between our measurements; mine agrees with that obtained by Mr. Brooke, and I believe also by Mr. W. Phillips. This difference will of course change most of the angles calculated by M. Rose, but not so materially as to make it necessary to trouble you with the result of my own calculations.

The flat crystals from St. Gothard are, as M. Rose had suspected, cleavelandite. It was indeed the observation of specimens of that locality which are not hemitrope, as most crystals of that substance are, that led me to the distinction of the two substances; and which gave me the best data for the determination of the primitive form. Mr. Turner's collection contains a great variety of forms of that locality; one of the most complicated I have represented in fig. 10. In some of the crystals, the planes I have marked $d^{\frac{3}{2}}$ and d' are wanting, and then the crystal has precisely the same form as some of the varieties of felspar. In the same collection are found crystals which are not hemitrope, from the Tyrol and from Siberia. Those from this last locality are very large, and contain only the modifications p m t and $o^{\frac{3}{2}}$ or o^2, and the figure of the plane m is triangular. However, most of the crystals are hemitrope, but their form is generally much more complicated than those M. Rose has figured. He says in his paper this substance is never found laminar, but from North America, and from Silesia. I have seen specimens in large laminæ, each of which is formed by the juxta-position of two laminæ parallel to the face T of the primitive, so as to present, when cleaved parallel to P, the same re-entering angles offered by the hemitrope crystals of that substance.

I shall feel obliged if you can spare room for a short description of, I believe, a very scarce and new mineral from Vesuvius. I have observed it upon a specimen Mr. Heuland purchased at the sale of Mr. Desse, to add to his private collection. This substance occurs in small brilliant colourless and translucent crystals. They are sufficiently hard to scratch rock crystal. Mr. Children, who kindly undertook to examine a small quantity of it, found it to be mostly composed of silex and magnesia. The only form I have observed is represented by fig. 12, and the crystals cleave easily in the direction of the plane p. The angles I have measured with the reflecting goniometer led me to adopt for the primitive form of this substance, a right rhombic prism, fig. 11, the lateral planes of which correspond to the planes marked m in fig. 12, and the base to the cleavage. The incidence of the two lateral planes of the primitive is 128° 54′, and the ratio of one side of the base to the height nearly that of 4 to 7. The other incidences are:

$$(b', p) = 126^\circ \ \ 6'$$
$$(b', g) = 110 \ \ 23.$$

This substance is accompanied by pleonast and olive-green pyroxene.

'I have chosen for it the name of *forsterite*, in honour of the late Mr. Forster, who has so much contributed to the advancement of mineralogy by his extensive connections in that branch of science in every part of the world, and by having laid the foundation of one of the finest private collections, now in the possession of Mr. Heuland.

Article XIII.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1823. Part II.

The following are the papers contained in this unusually voluminous part of the Philosophical Transactions.

XIII. *On a new Phenomenon of Electromagnetism.* By Sir Humphry Davy, Bart. Pres. RS.

We have reprinted this communication in the present number of the *Annals.*

XIV. *On Fluid Chlorine.* By M. Faraday, Chemical Assistant in the Royal Institution. Communicated by Sir H. Davy.

In the next number of the *Annals,* we intend giving a full account of the contents of this paper, as well as of another, by the same chemist, on the Liquefaction of other Gases.

XIV. *On the Motions of the Eye, in Illustration of the Uses of the Muscles and Nerves of the Orbit.* By Charles Bell, Esq. Communicated by Sir H. Davy.

A brief abstract of this valuable paper will be found in the report of the proceedings of the Royal Society in the *Annals* for May, 1823; but we extract the section "*On the two conditions of the eye, its state of rest, and of activity,*" on account of the peculiarly important nature of its contents.

"The eye is subject to two conditions: a state of rest with entire oblivion of sensation, and a state of watchfulness, during which both the optic nerve and the nerve of voluntary motion are in activity. When the eye is at rest, as in sleep, or even when the eye-lids are shut, the sensation on the retina being then neglected, the voluntary muscles resign their office, and the involuntary muscles draw the pupil under the upper eye-lid: This is the condition of the organ during perfect repose.

" On the other hand, there is an inseparable connexion between the exercise of the sense of vision and the exercise of the voluntary muscles of the eye. When an object is seen, we enjoy two senses: there is an impression upon the retina; but we receive also the idea of position or relation which it is not the office of the retina to give. It is by the consciousness of the degree of effort put upon the voluntary muscles, that we know the relative position of an object to ourselves. The relation existing between the office of the retina and of the voluntary muscles, may be illustrated in this manner.

" Let the eyes be fixed upon an illuminated object until the retina be fatigued, and in some measure exhausted by the image, then closing the eyes, the figure of the object will continue present to them: and it is quite clear that nothing can change the place of this impression on the retina. But notwithstanding that the impression on the retina cannot be changed, the idea thence arising may. For by an exertion of the voluntary muscles of the eye-ball, the body seen will appear to change its place, and it will, to our feeling, assume different positions according to the muscle which is exercised. If we raise the pupil, we shall see the body elevated, or if we depress the pupil, we shall see the body placed below us; and all this takes place while the eye-lids are shut, and when no new impression is conveyed to the retina. The state of the retina is here associated with a consciousness of muscular exertion; and it shows that vision in its extended sense is a compound operation, the idea of position of an object having relation to the activity of the muscles.

" We may also show, by varying this experiment, that an agitated state of the muscles, or a state of action where the muscles are at variance or confused, affects the idea of the image. If we look on the luminous body so as to make this impression on the retina, and then cover the face so as to exclude the light, keeping the eye-lids open, and if we now squint, or distort the eyes, the image which was vividly impressed upon the retina instantly disappears as if it were wiped out. Does not this circumstance take place, because the condition of the muscles thus unnaturally produced, being incongruous with the exercise of the retina, disturbs its operation?

" If we move the eye by the voluntary muscles, while this impression continues on the retina, we shall have the notion of place or relation raised in the mind; but if the motion of the eye-ball be produced by any other cause, by the involuntary muscles, or by pressure from without, we shall have no corresponding change of sensation.

" If we make the impression on the retina in the manner described, and shut the eyes, the image will not be elevated, although the pupils be actually raised, as it is their condition to be when the eyes are shut, because there is here no sense of voluntary exertion. If we sit at some distance from a lamp which has a cover of ground glass, and fix the eye on the centre of it, and then shut the eye and contemplate the phantom in the eye; and if, while the image continues to be present of a fine blue colour, we press the eye aside with the finger, we shall not move that phantom or image, although the circle of light produced by the pressure of the finger against the eye-ball moves with the motion of the finger.

"May not this be accounted for in this manner: the motion produced in the eye-ball not being performed by the appropriate organs, the voluntary muscles, it conveys no sensation of change to the sensorium, and is not associated with the impression on the retina, so as to affect the idea excited in the mind? It is owing to the same cause that, when looking on the lamp, by pressing one eye, we can make two images, and we can make the one move over the other. But, if we have received the impression on the retina so as to leave the phantom visible when the eye-lids are shut, we cannot, by pressing one eye, produce any such effect. We cannot, by any degree of pressure, make that image appear to move, but the instant that the eye moves by its voluntary muscles, the image changes its place; that is, we produce the two sensations necessary to raise this idea in the mind; we have the sensation on the retina combined with the consciousness or sensation of muscular activity.

"These experiments and this explanation of the effect of the associated action of the voluntary muscles of the eye-ball, appear to me to remove an obscurity in which this subject has been left by the latest writers. In a most scientific account of the eye and of optics, lately published, it is said on this question, 'we know nothing more than that the mind residing, as it were, in every point of the retina, refers the impression made upon it, at each point, to a direction coinciding with the last portion of the ray which conveys the impression.' The same author says, 'Kepler justly ascribed erect vision from an inverted image to an operation of the mind, by which it traces the rays back to the pupil, and thus refers the lower part of the image to the upper side of the eye.' What can be here meant by the mind following back the ray through the humours of the eye? It might as well follow the ray out of the eye, and, like the spider, feel along the line. A much greater authority says we puzzle ourselves without necessity. 'We call that the lower end of an object which is next the ground.' No one can doubt that the obscurity here is because the author has not given himself room to illustrate the subject by his known ingenuity and profoundness. But it appears to me, that the utmost ingenuity will be at a loss to devise an explanation of that power by which the eye becomes acquainted with the position and relation of objects, if the sense of muscular activity be excluded, which accompanies the motion of the eye-ball.

"Let us consider how minute and delicate the sense of muscular motion is by which we balance the body; and by which we judge of the position of the limbs, whether during activity or rest. Let us consider how imperfect the sense of touch would be, and how little of what is actually known through the double office of muscles and nerves, would be attained by the nerve of touch alone, and we shall be prepared to give more importance to the recti muscles of the eye, in aid of the sense of vision: to the offices performed by the frame around the eye-ball in aid of the instrument itself."

A plate accompanies this communication, showing the muscles of the eye as seen in front, and in profile.

(To be continued.)

Article XIV.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

The first meeting of this Society for the present session took place on the 20th of November last, when Major Gen. Sir G. Murray and John Rennie, Esq. were admitted Fellows; and the Croonian Lecture was read,

On the Anatomy of the Human Brain as compared with that of Fishes, Insects, and Worms; by Sir E. Home, Bart. V.P.R.S.

This lecture was very short, and consisted, principally, of remarks illustrative of the microscopical drawings by Mr. Bauer, with which it was accompanied, some more particular observations being reserved for the explanation of them. Occasion was taken to award a high and just tribute to the microscopical investigations of Swammerdam, which were unequalled, by any, it was remarked, except those of Mr. Bauer. The ability of both observers was of such and so rare a nature, that, with respect to each, it had been ascribed to some particular construction of the microscope; and it had even been suspected that Swammerdam had a peculiar method of using the instrument, which had died with him.

A portion of very recent human brain, merely steeped in distilled water, was examined by Mr. Bauer, who perceived in it rows of globules proceeding in straight lines from the cortical into the medullary part. A comparison was instituted of the human brain with the same organ in fishes, insects, and worms. In the tench, the brain has a central cavity, and its basis is nodulated. In the bee, that organ is larger in proportion than in the other insects which have been examined; it is also large in the moth and in the caterpillar.

The reading was commenced, likewise, of Some Observations on the Migration of Birds; by the late Dr. Edward Jenner, F.R.S.; communicated by his Nephew, Mr. H. C. Jenner.

Nov. 27.—Dr. D. Cresswell and Prof. Barlow were admitted Fellows of the Society; and the reading of Dr. Jenner's paper was concluded.

Dr. Jenner had intended to present this paper to the Royal Society himself, but was prevented from fully completing it, as to arrangement, by his extensive correspondence on the subject of vaccination. It commences with some general observations on the Migration of Birds, and particularly with respect to their capability of taking such great flights as migration must require, and which some writers have questioned. Dr. Jenner brings forward various facts, to show that there are no grounds for such doubt; among which are the following: a hobby-hawk was seen in a vessel near Newfoundland; and an owl, seemingly the common brown owl, flying above the Atlantic wave, with as much agility as if pursuing a mouse in the fields; cuckoos, snipes, and other birds, have likewise been seen in the Atlantic; a flock of birds resembling linnets settled on the rigging of a ship, remained awhile chirrupping in concert, and then flew away; geese have been caught in Newfoundland with their crops full of maize, a species of corn which is not grown but at an immense distance from that island. The discussion of this branch of the subject is succeeded by some remarks on the faculties of discrimination and guidance which must be exercised by birds, in the long flights thus taken, and which, Dr. J. conceives, must be of some peculiar and unknown nature; pigeons, it is observed, which have been taken several hundred miles, completely secluded from the light, by being shut up in a box, will, when set at liberty, immediately return to the place whence they were taken. The periodical disappearance and return of birds has been ascribed to hybernation, but of this Dr. Jenner never witnessed an instance; nor could he ever obtain any satisfactory evidence of it. When birds appear for the season, they are never in the emaciated and weakened state attended with loss of fat, seen in hybernating quadrupeds when they quit their retreats; but, on the contrary, they are quite vigorous, and as active as at any period. With regard to the supposed immersion of birds in ponds and rivers for the winter, Dr. J. remarks, that their respiratory organs are very similar in structure to those of quadrupeds, and are no better adapted for performing their functions under water. He took a swift, about the 10th of August, or on the eve of its departure, and held it under water, when it died in two minutes. It has been conjectured, that repeated alternate immersions and emersions might have the effect of altering the corresponding action of the heart and lungs; but though swifts and martins, it is observed, in reply to this conjecture, frequently splash in the water over which they are skimming, yet they never immerge themselves in it, and indeed if they were to do so, their wings would become so wet as to prevent their flying. The common duck, when pursued and forced to dive repeatedly, by a water-dog, arrives at the surface again much exhausted; as is likewise the case with grebes and auks, after repeated diving. Dr. Jenner had been in the habit of receiving Newfoundland dogs from that country, and had ascertained that they never continued under water for more than thirty seconds, and even then seemed confused when they came up. It had been asserted that negro and other divers remained under water several minutes; but Dr. J. conceives this assertion to be grounded only on a vague guess, and that the time was not measured by a stop-watch.

The next division of the paper relates to the remarkable effect of instinct in birds, of their returning to build on the same spot for many successive seasons. The author took twelve swifts from their nests in a barn, indelibly marked them all, by taking off two claws from one foot of each, and then set them at liberty. Some of them were caught again on the same spot, at the expiration of a year, and others after two years had elapsed; they were not attended to afterwards, but at the expiration of seven years from their original capture, one of these marked swifts was brought in by a cat.

Dr. Jenner next proceeds to state, as the cause of the migration of birds, that the tumid and enlarged state of the testes in the male, and of the ovaria in the female, at the season of their departure, prompt the animals to seek those countries where they can obtain proper succours for their offspring;—that, in fact, the nestlings are the objects of this provision. The parent birds leave the countries they migrate from at a time when their own wants are completely supplied; and they remain in those to which they migrate, no longer than suffices for the rearing of their young. Thus the swifts arrive in this country about the 5th or 6th of April, and depart hence about the 10th of August.—Dr. Jenner here observes, as a remarkable circumstance, that Ray, who attributed the migration of fishes to its true cause, that of seeking proper situations for spawning, overlooked the corresponding impulse as actuating birds.—The martins leave this country successively, some continuing to rear a brood much later than others: many of these birds roost in the walls of Berkeley Castle; and Dr. Jenner found, by dissecting a number, taken at the same time, that the ovaria of the females were in a variety of states; in some the eggs being no bigger than hemp seed, while in others they were as large as peas; the testes of the males exhibited analogous degrees of tumidity.

Swallows are seen flying over pools and waters in spring, in search of the gnats on which they are then obliged to food; and not because they have arisen from the waters. Their usual food, like that of swifts and martins, is a species of scarabæus, as the author ascertained by dissection.

Birds that rear several broods in the season, frequently leave the last brood to perish; thus a pair of swifts that had brought up three broods in one nest left the fourth to perish; and the mother came back in the following year, threw out the skeletons, and laid in the nest again. Many nests of late birds, of various species, are deserted in this manner by the parent animals; but the latter thus leave the country when it abounds with their own food.

The young birds, it is remarked, cannot be directed in their migratory flights by the parents, but must be guided by some unknown principle: if it be admitted in the case of swifts, martins, and other birds associating together in flocks, that the young may be directed by the motions of their fellows, yet this cannot be the case with the nightingales; nor with the cuckoos, who, though reared in the nests of many different birds, are regular migrators. The parent cuckoo has left the country before its young are reared, always departing early in July.

Dr. Jenner next gives some particulars relative to the enlargement of the testes and ovaria in birds, supplementary to those which have been pointed out by Mr. John Hunter. In those birds who pair but for a short time the testes are small, while in those with whom the connubial compact is of long continuance, they are large. In the cuckoo, a polygamist, and who continues with the female but for a very short time, the testes are of the size of a vetch only; but in the wren, whose attachment to his mate extends from spring to autumn, they are equal to a pea in magnitude; thus much larger in the latter than in the former, in proportion to the size of the bird. A continued supply of generative power is required in birds who pair for a long time, in case the brood should be destroyed—but in those like the cuckoo this provision is unnecessary.

The *winter birds of passage* leave this country for precisely the same reason that impels the *spring migrators* to come hither; some of them, as the wild-duck and the wood-pigeon, which occasionally build here, are irregular in their migration; the most regular are the red-wing and the field-fare, of whose building in this country Dr. Jenner never met with an instance. The food of the former, he observes, is not haws, or the fruit of the white thorn, as has been stated, but worms and insects, which they gather from the ground, feeding in flocks; Dr. J. had seen them dying of famine when haws were abundant. A gentleman saw a flock of field-fares on the day before the thawing of the great frost of 1794, and they seemed as wild and vigorous as if in season; he shot one, which Dr. Jenner examined, and found to be in excellent condition, but there was no food in the stomach, and the last which the animal had eaten was digested: now as the ground was covered with snow, and as the long frost had destroyed everything they could feed on, these field-fares must have returned here for a short time, in consequence of the inclemency of the weather abroad. Red-wings and field-fares always leave this country when they are in the best condition. The approach of severe frost is indicated by the arrival of water-birds, as that of thaw is by the coming of the spring migrators. Birds often outstrip in their migrations the progress of the frost itself.

Dr. Jenner considers that Dr. Darwin must be mistaken in what he says respecting cuckoos seen feeding their young. The birds in question must have been goat-suckers, which are very easily confounded with cuckoos by those who are not fully conversant with the characters of their plumage, &c.

This very interesting paper concludes with a recapitulation of the principal facts contained in it, and of the author's views respecting them.

Dec. 1.—The anniversary meeting took place this day (St. Andrew's Day falling on a Sunday), and was numerously attended.

After stating the names of those Fellows whom the Society has had the misfortune to lose since the last anniversary, the President, Sir H. Davy, delivered a discourse, in which he noticed such of them as had by their communications to the Society, or by their philosophical labours, advanced the progress of science. In presenting the following sketch of the President's address, we wish it to be distinctly understood that we pretend to offer a mere outline; it is quite impossible, in the space to which we are necessarily confined, to impart to the reader an idea of the high and eloquent eulogium which the President bestowed upon the memory and labours of some of the deceased Fellows.—Beginning with Dr. Hutton, he observed, that his labours of more than half a century had established his reputation as one of the most able mathematicians of his country and age; after alluding to the papers which had been published in the Transactions of the Society, he observed, that during the long period that he was Professor at Woolwich, he might be regarded as having eminently contributed to awaken and keep alive that spirit of improvement among the military students, which has so much contributed to the character of the British officer, and which has been attended with such beneficial results to the country. The merits of Dr. Hutton as an experimental philosopher, the President observed, were of no mean kind; they were displayed in his paper on Gunnery, for which he received the Copleyan medal, in 1778: this paper contained an account of some difficult and delicate experiments on the force of gunpowder, from which conclusions were drawn connected with important practical results. Sir Humphry then observed, that Dr. Hutton's greatest work was, perhaps, his calculation of the Density of the Earth, founded upon Dr. Maskelyne's experiments on the effects of Schehallien on the Plumb-line. This labour, comprehending the most complicated arithmetical processes, the President observed, would for ever associate his name with one of the grandest and most important physical problems solved in the last century, and transmit it with honour to posterity.

To speak of Dr. Edward Jenner as a man of science of our own particular school, the President observed, would be saying little, for he had a higher claim to our deep regret and profound admiration as a benefactor to mankind in general.—After adverting to the invention and effects of vaccination, Sir Humphry Davy remarked, that the originality of Dr. Jenner's mind and the accuracy of his observation are shown in his first communication to the Society, on the Natural History of the Cuckoo; and in the pursuit of his great object, he met with obstacles which required no ordinary degree of perseverance, and of confidence in his own powers to overcome; the fair way of judging of the merits of an inventor, said Sir Humphry, is by the operation of his discovery on civilized and social life;—and in this respect Dr. Jenner stands almost alone.

Of Dr. Baillie, the President observed, that whether considered as a physician or as a man, his talents and his virtues were alike distinguished,—his works show the accuracy and coolness of his judgment; his minuteness in observation; and his acuteness in referring effects to their true causes, amidst the complicated phenomena offered by diseased organs. No man was ever more free from any taint of vanity or affectation; he encouraged and admired every kind of talent, and rejoiced in the success of his contemporaries; and he maintained, even at court, the simplicity and dignity of his character.

Col. Wm. Lambton, the President observed, was a veteran in the army of India: two papers of his are published in the Transactions of the Society, on the Admeasurement of an Arc of the Meridian in Hindostan—a work of great labour, displaying minute accuracy and extraordinary perseverance, and carried on in a climate unfavourable to bodily exertion or intellectual pursuit. This arc extends in amplitude very nearly ten degrees; and Col. Lambton had the honour of having laid down the largest single arc ever measured upon the surface of the globe.

The President, when noticing Archdeacon Wollaston, observed that the little which he had contributed to the Society's Transactions occasioned regret that he had not been a more frequent contributor.—His papers, said Sir Humphry, on the Measurement of Heights, and on the Alteration of the Boiling Temperature, offer a valuable resource in ascertaining the altitudes of mountains, and are remarkable for accuracy of method and distinctness of detail.

After making respectful mention of Dr. Cartwright and Mr. Jordan, the President proceeded to make some observations on the award of the Copleyan medal, to John Pond, Esq. Astronomer Royal, for his various observations and communications published by the Royal Society; we can give a still fainter idea of this discourse, than of the tributes of praise to the deceased members: it was received by the Society in a manner which evinced their strong desire that it should be made a permanent record by the press.

Having given an historical sketch of the labours of the Astronomer Royal, and stated his merits as an accurate and indefatigable observer; the President observed, that it is very difficult to point out the specific merits of astronomical observations; they are not, he said, like philosophical or chemical experiments, which produce an immediate result; their delicacy and exactness, he observed, could only be judged of by those who have witnessed the manner in which they are made, and who are accustomed to the same kind of labour; and as they often relate to long periods of time, their correctness and value perhaps can only be fairly estimated by posterity.

The President then took a rapid but luminous historical view of the labours of Flamstead, Bradley, and Maskelyne, and he alluded to the discussion still pending between the Astronomer Royal and Dr. Brinkley on the subject of parallax.

Sir Humphry then adverted to the principal points of discussion in the papers of the Astronomer Royal, viz. the grand and long agitated question of the parallax of the fixed stars, and an apparent declination or change of position in a number of the stars, not to be accounted for by any known laws. He said the Council did not mean by this token of their respect for Mr. Pond, to give any opinion on the subject of parallax, which, however, it was satisfactory to find, was now brought into very narrow limits; nor did they enter at all into the subject of the *apparent declination*, for on a matter of such great importance, new observations, and the researches of years, were required to fix the judgment of scientific men.

Having mentioned the advantages which navigation has acquired from astronomical observations, and which to this country were peculiarly necessary, on account of its maritime and colonial empire, the President observed, that astronomy had exerted a powerful effect in the general improvement of the human mind, by developing the true system of the universe. In consequence of the discoveries made in it, all the superstitious notions—all the prejudices respecting the heavenly bodies, which had such an effect upon the destinies of individuals and of kingdoms in ancient times, have disappeared; and the science as it now exists is the noblest monument ever raised by man to the glory of his Maker; for its ultimate and refined developements demonstrate combinations which could only be the result of infinite wisdom, intelligence, and power.

On presenting the medal to the Astronomer Royal, the President addressed him nearly as follows:—I now present you this medal.—Consider it as a token of the respect of the Society, and of the confidence of the Council in the great accuracy of your observations: receive it likewise as a memorial that future important labours in the same department of science are hoped for, nay, are expected from you. I am well aware that some of the greatest and most important objects of discovery, and those, perhaps, most obvious, have been attained by the labours of your predecessors. Yet Nature is inexhaustible; and the powers and resources of the human mind, and the refinements of art, have not as yet attained their limits. Who would have anticipated, half a century ago, the discoveries of Herschel and Piazzi?

Though pursuing a science that may be considered as in its maturity, you have advantages of a peculiar kind; more perfect instruments than were ever yet employed; more extensive assistance than any of your predecessors; and upon these points the liberality and promptitude with which Government have entered into all the views of the Council of the Royal Society for the improvement of the Royal Observatory, cannot be too much admired. Continue to pursue your honourable career, and endeavour to be worthy of having your name transmitted to future generations with those of your illustrious predecessors. Of all the branches of science, astronomy is that from which this Society has gained most glory, and it never has lost, and I feel convinced never will lose, any opportunity of advancing its progress, and honouring its successful and zealous cultivators.

Article XV.

Scientific Intelligence, and Notices of Subjects connected with Science.

I. *Supposed Origin of the Art of Smelting Iron.*

The following remarks explanatory of a passage in the Rev. J. Hodgson's "*General Conclusions of an Inquiry into the Era when Brass was used for Purposes to which Iron is now applied,*" inserted in the last number of the *Annals*, at p. 407, were precluded from appearing in their proper place as a note to the paper, by circumstances attending their passage through the press.

It would appear, from a paragraph in the former part of his paper (Arch. Æl. vol. i. p. 40), that Mr. Hodgson conjectures the idea of producing metallic iron by the artificial application of fire to its ores, to have been suggested to mankind by the observation, that the stones containing malleable iron, or meteorites, descended upon the earth in an ignited state, or from fiery bodies in the atmosphere; though the application of the terms *aërolite* and *meteoric stone* to *meteoric iron* and *stones*, indiscriminately, render his remarks somewhat ambiguous. There really exists, however, an indeterminate kind of transition, from the masses of meteoric iron entirely free from earthy or stony matter, to the meteoric stones in which that metal is merely disseminated in grains. Thus, placing the Brazilian or Cape iron, and the Benares or L'Aigle stones at the extremities of the scale, the intermediate degrees will be formed by the Siberian iron, with its globules of (so called) meteoric olivine, the Elbogen iron in which globules of a similar substance are imbedded, and the stones which fell near Tabor, in Bohemia, in 1753, containing nearly one-fourth of their weight of iron. A sufficient quantity of the metal to impart a knowledge of its usefulness might have been separated from such stones as the latter, without much difficulty; and thus (allowing the validity of Mr. Hodgson's conjecture), mankind might have been led to the smelting of iron from its ores. It seems, indeed, that the Esquimaux inhabiting the western coast of Greenland, visited by Capt. Ross, actually edge their bone knives with small pieces of iron extracted from a meteoric stone, and flattened for the purpose. Mr. Hodgson is not the only writer who has attributed the first knowledge of metallic iron to the observation of native meteoric masses of that metal, for this idea has also been expressed by Mr. D. Mushet, in his article on Iron-making, in the Supplement to the Encyclopædia Britannica. The circumstance is somewhat remarkable, that the same extraordinary masses of iron, which, when first discovered, and even for a considerable subsequent period, were supposed by various writers to have resulted from ancient smelting operations, should now be considered as having pointed out to mankind the means of obtaining that metal by smelting.

Mr. Hodgson appears to have been misinformed with regard to the balls of iron-stone found in Sicily, which he alludes to: they certainly have no similarity *in substance* "to the true aërolites;" aërolites have no peculiar *shape*, but are extremely various and irregular in that respect; and the balls of iron-stone have no doubt received the appellation of thunderbolts for the same reasons, indirectly derived from a knowledge of meteorites, which induced different nations of antiquity to confer it on various other minerals, and even on certain organic remains. E. W. B.

II. *Composition of Ancient Bronze.*

The following particulars respecting ancient bronze are derived from two papers by the late Dr. E. D. Clarke, read before the Society of Antiquaries a few years since, and published in their Archæologia; but not hitherto transferred to any more general medium of scientific information.

In Dr. Clarke's "*Observations upon some Celtic Remains discovered near Sawston, seven miles from Cambridge*," Arch. vol. xviii. p. 340—343, he describes certain antiquities which had been found on the 3d of August, 1816, accompanying a human skeleton, about three feet below the surface of the ground, on the top of a small eminence called Huckeridge Hill. They consisted of two vessels of bronze, some fragments of the coarsest black terra cotta, an iron sword entirely converted into oxide, a massy bronze ring which had been the foot of the larger vessel, the iron umbo of a shield, a bronze broach or buckle, and a small iron fibula. There was nothing Roman in their character: the form of the sword, in the Rev. Mr. Kerrich's opinion, was not Roman; the fragments of terra cotta resembled those found with Celtic remains; and these circumstances, notwithstanding their being discovered near the Roman station upon the Gog Magog Hills, tended to show that they were not of Roman origin. The vessels' consisting of an alloy of copper and tin seemed likewise, in Dr. Clarke's opinion, to refer these remains to an earlier period than the time of the Romans in Britain.

Dr. Clarke found that the bronze of which the vessels were made, was composed of 88 parts of copper, and 12 of tin: he ascertained, also, that the bronze coins of Antoninus Pius and of Marcus Aurelius consisted of the same alloy.

In his "*Account of some Antiquities found at Fulbourn in Cambridgeshire*," Arch xix. 56—61, Dr. C. describes two swords, a spear-head, and two ferrules supposed to have been the feet of spears, which were found on Fulbourn Common early in 1817. They were all of bronze, the spear and swords formed on the Grecian model; a bronze sword resembling the latter had been taken out of the river Cam many years before, and swords of the same kind had been found in Ireland. The alloy was hard and brittle; its fracture, earthy, white, and destitute of metallic lustre, but upon filing showed the splendour and colour of gold; its specific gravity was 9·200; it consisted, like the bronze of the other relics, of 88 copper and 12 tin.

Dr. Clarke adverts, in the conclusion of this paper, to the "uniformity characterising all the results which different chemists have obtained in the analysis of ancient *bronze*; a degree of uniformity," he continues, "hardly to be explained without supposing that there may have existed a native compound of the two metals thus united. In almost every instance the proportion of the *copper* to the *tin* has been 88 to 12. This was the result of the analysis made by Mr. *Hatchett*, of the *bronze* nails brought by Sir *Wm. Gell* from the tomb of *Agamemnon* at *Mycenæ*; the same result was also obtained in the analysis by Dr. *Wollaston*, of some arrow-heads of *bronze* found in the *South of Russia*; and I have found the same constituents similarly combined in various specimens of *bronze* from *Grecian* and from *Celtic* sepulchres; in the *bronze* lamps of ancient *Egypt*, and in the *lares*, *weapons*, and other *bronzes* of the same country. That in the analysis of *bronze*, found in countries widely separated, there should not be a more perceptible difference in the proportion of their chemical constituents, is a remarkable circumstance. The *Gaulish* axe found in *France*, by M. *Dupont de Nemours*, and which cut wood like a *steel* axe, might be considered as an exception; because it contained, according to the analysis of *Vauquelin*, 87 parts of *copper* combined with 9 parts of *tin*; but in this *axe* there were also present 3 parts of *iron*; perhaps an impurity of the *tin*; which is rarely free from an admixture of other metals. The *tin* of the *Fulbourn* swords, when exposed to a violent heat, yielded an alliaceous smell denoting the presence of *arsenic*; and a very small portion of a black insoluble powder remained in the *nitric* acid after the solution of the *copper*.

"To conclude, therefore, if we may be permitted to consider these *bronze* reliques as so many characteristical vestiges of a peculiar people, to whom the art was known of giving a *maximum* of density to *copper* and *tin*, by a chemical operation, we shall be at a loss, either to ascertain their origin, or to account for their wide dispersion. Such reliques, as it has been proved, are found alike in *Egypt* and in *Greece*, in *Great Britain*, and in *Ireland*. To this it may be added, that the most ancient *bronze* coins of *India* (of which I have lately analyzed some that were found near the *Byzantium* of *Larice*, upon the *Barygazenus Sinus*), consist of a similar *alloy*; and I have reason to suspect that the *bronze* idols of *Tahtary*, and of *China*, will, upon a chemical examination, be found to contain the same ingredients."

III. *Parhelia, &c.*

The following is an account of parhelia and other phenomena observed at Darlington, in the county of Durham, on the 30th of Oct. 1823.

The writer saw it first at ten minutes past twelve.

In a line with the sun, and equidistant from it, were two bright spots coloured like the rainbow, from one of which came a stream of light in a horizontal direction. These spots appeared to be the termination of a bright semicircle, having the sun for its centre, and arching upwards. The most surprising part of the sight was another arc diverging contrarywise, having the same or a larger radius, and joining the other at the back, or outer side. The most beautiful part of the sight was another double arc, just like the one I have described, in or near the zenith; very bright, and having all the colours of the rainbow. The phenomenon, varying only in the degree of brightness, continued for three-quarters of an hour, and one of the spots remained ten minutes longer.

The sky was nearly or quite cloudless, and very misty; the wind due north. Light clouds soon made their appearance after the arcs disappeared.

A letter from the same observer, dated Nov. 17, 1823, gives further particulars, viz.

The two parhelia appeared on the external margin of the prismatic semicircle, at the two extremities of its horizontal diameter. The brighter one lasted the longest. The colours were not very well defined; yellow predominated. The arcs nearest the sun had the least of colour in them, being scarcely more than bright or luminous appearances; the more distant ones had a good deal of colour.

I also observed, which I think I did not before mention, about 90° from the sun, and about its altitude, a large faintly bright spot, and a light streak from it in a horizontal direction, both quite colourless: this was visible nearly as long as the rest of the phenomenon.

It would be interesting to know over what extent of country the phenomenon presented the same appearance. In some parts it might, perhaps, appear more perfect.

IV. *Effect of Heat in lessening the Cohesive Force of Iron.*

A bar of malleable iron, three feet in length, and one inch square, was heated to 212°, and the machine for measuring its flexure being in readiness, so that a weight of 300 lbs. could be instantly let down upon the bar; while at the same time the observer adjusted the index to zero. These operations having been effected in a close and warm room, with as little loss of heat as possible, the windows were thrown open, the heating bath removed, and the effect of cooling observed.

The flexure decreased as the bar cooled, and after it had remained two hours in order to be cooled down to the temperature of the room, which was 60°, the flexure had decreased three-fourths of one of the divisions of the scale; and when the weight was raised from the bar, it returned through 14 divisions. Hence we may conclude that by an elevation of temperature equal to 212° – 60° = 152 degrees, iron loses about a twentieth part of its cohesive force, or will bend one-twentieth more by the same load. This is equal to about a 3000th part for each degree.—(Tredgold on Cast Iron, 2d Edit. p. 104.)

V. *Correctness of Greenwich Observations.*

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR, *Blackman-street, Dec.* 20, 1824.

In the October number of the *Annals*, a notice was inserted by me, wherein it was stated, "a communication has, we understand, been received from Mr. Bessel, acknowledging that his catalogue of principal stars requires a correction for instrumental flexure, thereby admitting the superiority of the Greenwich one." It seems, however, that the accuracy of the report is by Messrs. Tilloch and Taylor contradicted;* upon what grounds, it is immaterial to inquire: but as a charge of misrepresentation is *insinuated*, I shall merely state, that a letter was sent by Dr. Tiarks (a German astronomer in the pay of the British Government), containing an extract of a letter (translated into English) which he had received from Mr. Bessel, couched in such a manner as to induce the gentleman to whom it was addressed, to transmit to Mr. Troughton a note, informing him of Mr. Bessel's concession; and which note was shown to *me*, as well as to *many others*, interested in these matters. Not, however, content with having done thus much, Dr. Tiarks subsequently called upon Mr. Troughton, and with much apparent satisfaction, personally communicated to him the same concession on the part of his friend; and among other things said, "Bessel had acknowledged that had he used Pond's mode of observing sooner, he should have gotten his latitude *correctly*." And at this time there can be no doubt but that Dr. Tiarks considered himself justified in promulgating Mr. Bessel's acknowledgment of the superior accuracy of the Greenwich catalogue. What new light may have since broken in upon this gentleman, I do not pretend to know; it is right, however, the readers of the *Annals* should be apprised, that the communication was not made to them upon slight grounds. Mr. Bessel also should be informed, that whatever "*idle reports*" (if such they at present be) have gone abroad, are of *German*, not of *British* origin; and that they have been circulated by the industry of his own friend, and from a letter of his own writing.

JAMES SOUTH.

VI. *British Museum and Edinburgh Review.*

The author of an article in the Edinburgh Review, on the British

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* Philosophical Magazine for November last.

Museum, would feel much obliged to the Editor of the *Annals of Philosophy*, if he would permit him, through the medium of the *Annals*, to correct a few errors which have crept into the above article, and which might be considered as instances of bad faith or ignorance were they not acknowledged to proceed from the hurry of the moment, and the want of an opportunity of correcting the proof sheet.

Page 382.—The price of the Elgin marbles is stated at 8000*l*.; but it should have been 6000*l*.

Page 385.—Murex *Carinatus* is misprinted *M. carinalus*.

Page 389.—The paragraph beginning "The purchases made two or three years ago by Dr. Leach," should have run thus:—"The purchases made several years ago for the Museum included some extremely rare and splendid trochili, or humming birds, some of which would bring three or four guineas a piece."

Page 390, line 18.—"This immense herbarum," should have been, "His immense, &c."

Page 390.—The trustees are said to be 41; but they are now 43.

Page 391.—They should have been stated at 21 official trustees, including the three principal Secretaries of State; 7 family trustees, of which 1 represents the family of Sloane, 2 that of Cotton, 2 that of Harley, 1 that of Townley, and 1 Lord Elgin. The elected trustees are 15, making in all 43.

There are a few minor errors of trifling importance, because they do not affect the accuracy of the statements: but the author has the satisfaction of knowing that his strictures have produced a *sensation* in the quarter where he most desired it; and the next opening of the Museum will convince the public that his animadversions have produced beneficial effects. He has been the cause of the destruction of numberless moths; and some of the insect treasures of the Museum have been recently brought to light. He had but one object—to call public attention to a great abuse; and if his zeal for the cultivation of a favourite study has betrayed him into warmth of expression, he hopes that he has indulged in no unbecoming personalities.

An Old Correspondent.

Article XVI.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

M. de la Beche will shortly publish a Selection of the Geological Memoirs contained in the Annales des Mines, together with a Synoptical Table of Equivalent Formations, and M. Brongniart's Table of the Classification of Mixed Rocks: in 1 vol. 8vo.

Mr. C. Chatfield has in the press a Compendious View of the History of the Darker Ages, with Genealogical Tables; to form 1 vol. 8vo.

A Guide to the Mount's Bay and the Land's End, comprehending the Topography, Botany, Agriculture, Fisheries, Antiquities, Mining, Mineralogy, and Geology, of Western Cornwall: Second Edition. Illustrated by Engravings on Copper and Wood. By a Physician. To form 1 pocket volume.

Article XVII.

NEW PATENTS.

Sir W. Congreve, of Cecil-street, bart. Strand, for various improvements in fire-works.—Oct. 16.

A. Buchanan, of Cathrine Cotton Works, Glasgow, merchant, for his improvement in the construction of weaving looms impelled by machinery.—Oct. 16.

J. Ranking, Esq. New Bond-street, Westminster, for his newly invented means of securing valuable property in mail and other stage coages, travelling carriages, waggons, caravans, and other similar public and private vehicles, from robbery.—Nov. 1.

G. Hawkes, Lucas-place, Commercial-road, Stepney Old Town, Middlesex, ship-builder, for his improvement in the construction of ship anchors.—Nov. 1.

G. Hawkes, Lucas-place, Commercial-road, Stepney Old Town, Middlesex, ship-builder, for certain improvements on capstans.—Nov. 1.

W. Burdy, Fulham, mathematical-instrument maker, for his anti-evaporating cooler to facilitate and regulate the refrigerating of werts, or wash, in all seasons of the year, from any degree of heat between boiling and the temperature required for fermenting.—Nov. 1.

T. F. Gimson, Tiverton, Devonshire, Gent. for various improvements in addition to machinery now in use for doubling and twisting cotton, silk, and other fibrous substances.—Nov. 6.

T. Gowan, Fleet-street, London, truss-manufacturer, for certain improvements on trusses.—Nov. 11.

J. Day, Esq. of Barnstaple, Devonshire, for certain improvements in percussion gun-locks applicable to various descriptions of fire-arms.—Nov. 13.

J. Ward, Grove-road, Mile-End-road, Middlesex, iron-founder, for certain improvements in the construction of locks and other fastenings.—Nov. 13.

S. Servill, of Brower's Hill, Bisley, Gloucestershire, clothier, for his new mode or improvement for dressing of woollen or other cloths.—Nov. 18.

R. Green, Lisle-street, St. Anne, Middlesex, sadlers' ironmonger, for certain improvements in constructing gambadoes or mud-boots, and attaching spurs thereto, and part of which said improvements are also applicable to other boots.—Nov. 13.

R. Stein, Tower Brewery, Tower-hill, brewer, for his improved construction of a blast-furnace, and certain apparatus to be connected therewith, which is adapted to burn or consume fuel in a more economical and useful manner than has been hitherto practised.—Nov. 13.

J. Gillman, Newgate-street, silk warehouseman, and J. H. Wilson, Manchester, silk and cotton manufacturer, for certain improvements in the manufacture of hats and bonnets.—Nov. 18.

J. Heathcoat, Tiverton, Devonshire, lace-manufacturer, for a machine for the manufacture of a platted substance composed either of silk, cotton, or other thread or yarn.—Nov. 20.

ARTICLE XVIII.

METEOROLOGICAL TABLE.

1823.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.	Daniell's hyg. at noon.
11th Mon.								
Nov. 1	N W	30·22	29·86	45	27	—		
2	N W	30·23	30·16	46	25	—		
3	N W	30·16	29·83	45	32	—		
4	S W	29·83	29·74	49	44	—	21	
5	E	30·03	29·74	50	44	—	19	
6	E	30·05	30·03	58	48	—	44	
7	N W	30·27	30·05	55	50	—	27	
8	N W	30·50	30·27	56	38	—		
9	N E	30·61	30·50	46	32	—		
10	E	30·68	30·61	46	29	—		
11	E	30·68	30·57	42	23	—		
12	E	30·57	30·54	40	21	—		
13	S	30·54	30·44	43	23	—		
14	S W	30·44	30·35	44	32	—		
15	N W	30·49	30·36	50	36	—		
16	N	30·50	30·49	51	33	·51		
17	N W	30·52	30·50	42	37	—	—	
18	N E	30·51	30·33	48	42	—		
19	E	30·33	30·18	46	40	—		
20	S W	30·28	30·17	51	40	—		
21	W	30·28	30·12	48	45	—		
22	S W	30·12	30·07	47	39	—		
23	S W	30·17	30·12	48	43	—		
24	S W	30·31	30·12	50	45	—		
25	S W	30·34	30·31	50	41	—		
26	N W	30·33	30·30	48	44	—		
27	S W	30·30	30·15	48	45	—		
28	S W	30·15	29·85	49	45	—	35	
29	S	29·85	29·60	52	44	—	13	
30	S W	29·79	29·59	57	50	·35	13	
		30·68	29·59	58	21	0·86	1·72	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Eleventh Month.—1. Cloudy. 2. Fine: white frost in the morning. 3. White frost; foggy. 4. Cloudy. 5. Rainy. 6. Fine. 7. Rainy. 8. Cloudy. 9. Fine. 10. Very fine. 11. Fine. 12. Fine: hoar frost. 13. Ditto. 14. Hoar frost: foggy: overcast. 15. Overcast. 16. Very fine. 17. Overcast: a little rain in the morning. 18, 19. Overcast. 20. Fine. 21—24. Overcast. 25. Foggy morning: overcast. 26, 27. Overcast. 28. Cloudy. 29. Rainy. 30. Cloudy.

RESULTS.

Winds: NE, 2; E, 6; S, 1; SW, 12; W, 1; NW, 8.

Barometer:	Mean height	
	For the month	30·233 inches.
	For the lunar period, ending the 25th	30·183
	For 15 days, ending the 13th (moon south)	30·145
	For 12 days, ending the 25th (moon north)	30·308
Thermometer:	Mean height	
	For the month	43·115°
	For the lunar period	42·550
	For 30 days, the sun in Scorpio	42·483
Evaporation		0·86 in.
Rain		1·72

Laboratory, Stratford, Twelfth Month, 22, 1823. R. HOWARD.

Fig. 1.

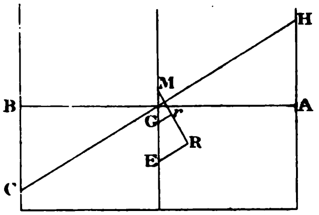


Fig. 2.

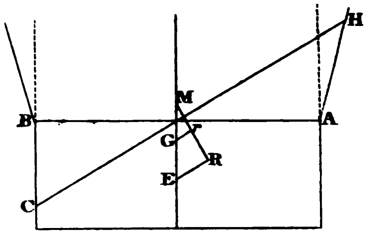


Fig. 3.

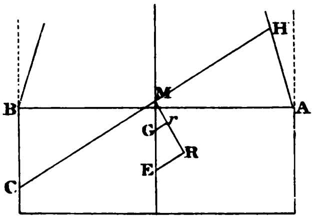


Fig. 4.

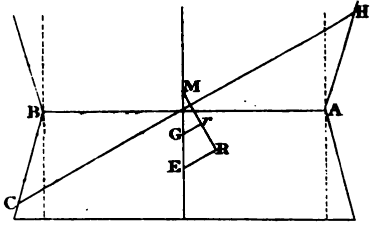


Fig. 5.

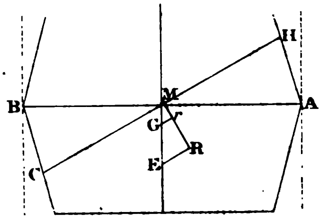


Fig. 6.

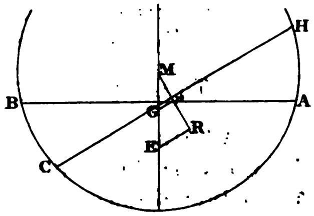


Fig. 7.

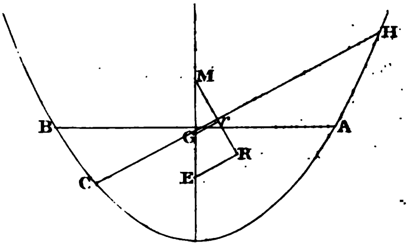


Fig. 8.

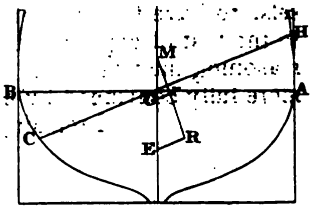


Fig. 9.

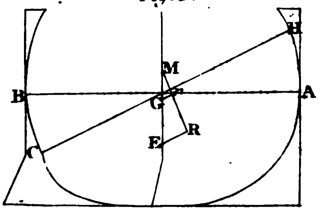
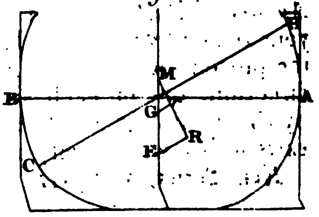


Fig. 10.



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ANNALS

OF

PHILOSOPHY.

FEBRUARY, 1824.

ARTICLE I.

Experiments on the Stability of Floating Bodies. By Col. Beaufoy, FRS. (With a Plate.)

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Bushey Heath, Jan.* 1, 1824.

IN the year 1798 a disquisition on the stability of ships by that celebrated and eminent mathematician the late Mr. Atwood, was read before the Royal Society, and published in the Transactions. As this work is allowed to be superior to any other on the same subject in the English language, it may not be unacceptable to those concerned in the planning or building of vessels, to have this gentleman's theoretical deductions submitted to the test of experiment; for however satisfactory mathematical reasoning may be to the scientific, the generality of readers are more fully convinced by experimental proof.

I remain, dear Sir, yours very truly,
MARK BEAUFOY

The apparatus with which these experiments were made is similar to the one described and illustrated by a plate in the *Annals of Philosophy* for March, 1816, and to which I request your readers will refer. Improvements, however, were made on the present occasion, by fixing two pieces of wood in a diagonal direction, one on each side of the mast, to prevent its bending by the inclining weight: and for greater security, their lower extremities were firmly tied to the transverse piece that connected the sides of the model, and through which the mast was inserted. A second alteration consisted in a more accurate adjustment of the centre of gravity at any given point of the figure when loaded. Raising the ballast (bars of lead) at first nearly produced this effect, which was afterwards determined with greater nicety by pouring shot into a cup fixed upon the top of the mast. This contrivance so well answered the intended purpose, that the results of several trials were found not to vary from each other more than five-hundreths of an inch.

Different formed bodies were used, measuring in breadth ten inches, and in length fourteen inches, or within a few hundredths of an inch of fourteen. The immersion in water, with the exception of the three last, was four inches, or two-fifths of the width. The total depths were various, those bodies whose sides projected outwards requiring greater depth than those with sides inclining inwards; and for this reason, the edge of the former when inclined becomes sooner level with the surface of the water.

Fig. 1 has the sides *parallel* to the plane of the mast both above and beneath the water line.

Fig. 2 has the sides projecting 15 degrees *outward* above the water line, and parallel to the masts under.

Fig. 3 has the sides inclining *inwards* 15 degrees above the water line, and parallel to the plane of the masts under.

Fig. 4 has the sides projecting *outwards,* and at equal inclinations (15 degrees both above and beneath the water line).

Fig. 5 has the sides inclining *inwards* and at equal inclinations (15 degrees) to the plane of the masts above and below the water line.

Fig. 6 has the sides coincident with the surface of *a cylinder,* the vertical sections being equal circles.

Fig. 7 has the vertical sections terminated by the *arcs of a parabola.*

Figs. 8, 9, 10, refer to the experiments made on the greatest vertical section (or midship bend) of an 18 gun brig, the Leopard of 50 guns, and the Howe of 120 guns.

In fig. 1 (Plate XXVI), B A is the surface of the water when the vessels float upright. C H the water line when they incline 30 degrees. E refers equally to the centres of gravity of the displaced fluid when the figure floats horizontally, and to the models in the first set of experiments. G is the centre of gravity of the model in the second set of experiments; the distance E G being 1·3 inches, or $\frac{13}{100}$ of the breadth B A; E R, and G *r,* are the lever on which the water acts to re-establish the vessel in a vertical position. M is the meta centre, or point below which the vessel's centre of gravity ought always to be situated to prevent its oversetting.

The displaced water, as well as the weights applied to incline the vessel, were calculated in ounces, drachms, and scruples, and afterwards reduced to the decimal parts of an ounce, three scruples in this case being equal to one drachm.

Example 1.—*Model* 1.—*Experiment* I.—The total depth of the model being 7·1 inches, the height of the centre of gravity is subtracted in each experiment, and the length of the mast (measuring from the centre of gravity of the model to the apex), 20·96 inches being afterwards added, gives the length of lever at which the weights are applied to produce the various inclinations of 5° 10°, 15°, 20°, 25°, and 30°, ~~Ther.~~ Thus The centre of gravity in Exper. 1 being situated two inches above the bottom of the model, and coinciding with the centre of gravity of the displaced fluid, the weight 2·2239 oz. being applied to the mast will incline it 5°. It is obvious that to restore the vessel to its original vertical position, the momentum of the water must be equal to that of the inclining power. If, therefore, the momentum of the effort to incline the vessel (that is to say, the weight applied, multiplied by the length of the mast) be divided by the weight of the displaced water, the quotient will be the length of lever E R on which the water acts.

Experiment I.—*Model* 1, inclined 5°.—The inclining weight is 2·2239 ounces, the length of lever 26·06 inches, and weight of the displaced water 324·52 ounces, then $\frac{2\cdot2239 \times 26\cdot06}{324\cdot52} = \cdot18$ or E R. By proceeding in a similar manner, the length of lever is obtained for 10°, 15°, 20°, 25°, and 30°. It should be mentioned, that after the vessel had been inclined on one side, it was turned and inclined on the other, by which means if the mast was not perpendicular, or there was any inaccuracy in the form of the model or manner of placing the ballast, it was immediately perceived, and the error corrected in taking the mean of the two experiments. The difference, however, seldom amounted to two drachms, and in general was much less.

The result of the second set of experiments proved the accuracy of the first, the altitude of the point M being nearly the same in both cases, as may be seen on reference to the annexed tables.

Column 1 shows the angles of inclination. Columns 2 and 6, the weights that produced that inclination. Columns 3 and 7, the length of lever E R and G *r*. Columns 4 and 8, the lever E R and G *r*, calculated by Mr. Atwood's theorem. Columns 5 and 9, the height of the point, M above the point E, and found in the following manner:—As the sine of the inclination : E R or G *r* : : radius : E M or G M. When the centres of gravity of figs. 1 and 7 coincide with the centres of gravity of the displaced fluid, their stability, allowing for the attendant inaccuracy of experiments, will be the same as shown by the subjoined calculations.

5	0·18 × 324·52 = 58	0·26 × 215·97 = 56
10	0·36 × 324·52 = 117	0·53 × 215·97 = 115
15	0·55 × 324·52 = 178	0·81 × 215·97 = 175
20	0·75 × 324·52 = 243	1·11 × 215·97 = 240
25	0·97 × 324·52 = 315	1·45 × 215·97 = 313
30	1·22 × 324·52 = 396	1·83 × 215·97 = 395

Figs. 8, 9, 10, are the greatest vertical sections of three men of war.

The object in making these experiments was to determine how much the meta centres of these parts of the ships are elevated above the load water line. Each figure was submitted to two experiments, for determining the height of the meta centre above the centre of gravity of the displaced fluid. These two were then added together, and the draught of water subtracted. For instance, the 18 gun brig 3·22 inches, the mean height of the meta centre (see Table VIII and Exper. 1), above the point E, being added to 2·24 inches. The centre of gravity gives 5·46 inches, from which deduct 3·60 the draught of water, and the remainder 1·86 will be the quantity sought. The beam of a man of war of this size is 30 feet, and $\frac{186}{1000}$ of 30 is 5·58 feet, which is the height of the brig's meta centre above the water when inclined between 15° and 20°. (See Table VIII.)

The mean height of the Leopard's meta centre above the centre of gravity of the water is 2·38, to which add 2·38, and from the total subtract 4·13, the remainder $\frac{63}{100}$ is the distance of the meta centre above the line of floatation, or $\frac{63}{1000}$ parts of the breadth. The beam or greater breadth of this 50 gun ship is 39$\frac{1}{4}$ feet, $\frac{63}{1000}$ of 39$\frac{1}{4}$, is 2·49 feet, the meta centre's height, which point may be considered as stationary.

The mean height of the meta centre of the Howe above the centre of gravity of the displaced water is 2·26 inches, to which add 2·43, and deduct 4·27 (the draught of water). The remainder $\frac{42}{100}$ parts of an inch is the meta centre's altitude above the water, or $\frac{4}{100}$ of the width. The greatest breadth of the 120 gun ship is 54 feet 5 inches, or 54·417; $\frac{4}{100}$ of 54·417 is 2·27 feet, the Howe's meta centre above the load water line. The meta centre being stationary, proves that the midship bends of the Howard and Leopard are in great measure circular.

The centre of gravity of the displaced water of the men of war was determined mechanically, and the columns in the tables headed Theor. are not filled up, it being impossible to give any general rule for finding the centre of gravity of bodies formed by different curves or mixed lines.

Although the theory of stability is perfect, yet the calculations are attended with considerable trouble, especially in complex forms, such as the hulls of ships, which require the labour of months. Under these circumstances is not the mechanical method adopted in these experiments of finding the meta centre by first suspending the model on pivots, and then inclining it in water by weight, the preferable mode of proceeding? I have reason to believe that with proper attention, and provided the pivots turn on friction rollers, the centre of gravity could be ascertained within the hundredth part of an inch.

A most essential point in ship building is the framing and putting together of the materials. This is now so well performed through the eminent skill and superior abilities of Sir Robert Seppings, as to render it doubtful whether this branch of naval mechanism admits of further extension. These observations, however, will not apply to another subject of naval science. Indeed it seems scarcely credible that in the first maritime nation in the world, one whose very existence depends upon its shipping, and upon the building of whose men of war millions and hundreds of millions have been expended from the time of Henry VIII. to the present day, no series of experiments on the resistance of fluids should have been undertaken by authority. Should this unaccountable neglect be palliated by the trite remark, that that which is found to answer in smooth water is inapplicable to rough, it may be answered, that Emerson in his octavo book of Mechanics, p. 113, speaking of a watch keeping time at sea, declares that "to suppose any regular motion can subsist among ten thousand irregular motions, and in ten thousand different directions, is a most glaring absurdity," yet notwithstanding the prediction of this celebrated writer, chronometers are found to be of most essential use, and as such taken on board most ships of value. Vessels propelled by steam now cross from Falmouth to Spain and back, yet not many years have elapsed since this mode of navigation was declared utterly impracticable.

The expence of making a complete set of experiments probably would not exceed the value of the main mast of a single line of battle ship; and a more convenient and eligible situation for conducting such experiments cannot be found than the King's Dock Yard at Woolwich. Such an undertaking is absolutely necessary for discovering the solid of the least resistance, and for the improvement of the hulls of vessels; and when the immense importance is considered of so easy and indispensable an elucidation of this interesting but ill understood branch of physics, it is hoped that the naval administration of my Lord Melville, which has already done so much for the promotion of science, will not consider the *resistance of fluids* as unworthy of notice. Every member of the community must participate in a wish that his Lordship may be pleased to issue directions for prosecuting an inquiry, of which the success cannot be doubted, if the investigation be committed to the skill, science, and zeal, which at present distinguish the Navy Office.

TABLE I.

Model 1.

Weight of water displaced 324·52 ounces.

Exp. 1. Lever 26·06 inches. Centre of gravity 2 inches.					Exp. 2. Lever 24·76 inches. Centre of gravity 3·3 inches.			
Deg.	Oz.	E R	Theor.	M C	Oz.	Gr.	Theor.	M C
5	2·2239	0·18	0·18	2·05	0.8333	0·06	0·07	2·03
10	4·4635	0·36	0·37	2·06	1·7569	0·13	0·14	2·07
15	6·8487	0·55	0·56	2·12	2·7777	0·21	0·22	2·12
20	9·2942	0·75	0·76	2·18	3·9583	0·30	0·31	2·18
25	12·089	0·97	0·97	2·30	5·4202	0·41	0·42	2·28
30	15·156	1·22	1·22	2·43	7·2464	0·55	0·56	2·41
1	2	3	4	5	6	7	8	9

TABLE II.

Model 2.

Weight of water displaced 324·52 ounces.

Exp. 1. Lever 26·66 inches. Centre of gravity 2 inches.					Exp. 2. Lever 25·36 inches. Centre of gravity 3·3 inches.			
Deg.	Oz.	E R	Theor.	M C	Oz.	Gr.	Theor.	M C.
5	2·2135	0·18	0·18	2·09	0·8437	0·06	0·07	2·06
10	4·5000	0·37	0·41	2·12	1·8437	0·18	0·18	2·13
15	7·0208	0·58	0·59	2·23	3·0156	0·23	0·25	2·21
20	9·7187	0·80	0·82	2·33	4·5156	0·35	0·37	2·33
25	12·932	1·06	1·08	2·51	6·4219	0·50	0·53	2·49
30	16·661	1·37	1·38	2·74	9·0625	0·71	0·73	2·72
1	2	3	4	5	6	7	8	9

TABLE III.

Model 3.

Weight of water displaced 324·52 ounces.

Exp. 1. Lever 26·10 inches. Centre of gravity 2 inches.					Exp. 2. Lever 24·80 inches. Centre of gravity 3·3 inches.			
Deg.	Oz.	E R	Theor.	M C	Oz.	Gr.	Theor.	M C
5	2·1929	0·18	0·18	2·02	0·8177	0·06	0·06	2·02
10	4·3073	0·35	0·35	1·99	1·5677	0·12	0·13	1·99
15	6·4062	0·52	0·53	1·99	2·3335	0·18	0·20	1·99
20	8·5312	0·67	0·71	2·01	3·1667	0·24	0·26	2·01
25	10·797	0·87	0·89	2·05	4·1198	0·31	0·34	2·04
30	13·208	1·06	1·08	2·12	5·2604	0·40	0·43	2·10
1	2	3	4	5	6	7	8	9

TABLE IV.

Model 4.

Weight of water displaced 250·14 ounces.

Exp. 1. Lever 26·71 inches. Centre of gravity 1·88 inch.					Exp. 2. Lever 25·41 inches. Centre of gravity 2·29 inches.			
Deg.	Oz.	E R	Theor.	M C	Oz.	Gr.	Theor.	M C
5	2·1937	0·16	0·17	3·80	0·6094	0·04	0·06	3·78
10	4·5625	0·34	0·35	3·88	1·5260	0·11	0·12	3·85
15	7·3229	0·54	0·56	4·03	2·8177	0·20	0·23	4·00
20	10·456	0·78	0·77	4·20	4·5469	0·32	0·33	4·17
25	14·427	1·07	1·09	4·47	7·0104	0·56	0·54	4·40
30	19·260	1·43	1·43	4·79	10·417	0·74	0·78	4·70
1	2	3	4	5	6	7	8	9

TABLE V.

Model 5.

Weight of water displaced 289·96 ounces.

Exp. 1. Lever 26·03 inches. Centre of gravity 2·06 inches.					Exp. 2. Lever 24·73 inches. Centre of gravity 3·36 inches.			
Deg.	Oz.	E R	Theor.	M C	Oz.	Gr.	Theor.	M C
5	2·1302	0·19	0·20	2·19	0·9375	0·08	0·08	2·22
10	4·1823	0·37	0·37	2·16	1·7448	0·15	0·14	2·16
15	6·0937	0·55	0·57	2·11	2·4896	0·21	0·23	2·12
20	8·0104	0·72	0·74	2·10	3·2083	0·27	0·29	2·10
25	9·9583	0·89	0·91	2·11	3·9896	0·34	0·36	2·10
30	11·875	1·07	1·09	2·13	4·7812	0·41	0·44	2·12
1	2	3	4	5	6	7	8	9

TABLE VI.

Model 6.

Weight of water displaced 240·74 ounces.

Exp. 1. Lever 25·73 inches. Centre of gravity 2·33 inches.					Exp. 2. Lever 24·33 inches. Centre of gravity 3·63 inches.			
Deg.	Oz.	E R	Theor.	M C	Oz.	Gr.	Theor.	M C
5	2·1719	0·23	0·24	2·66	1·1667	0·12	0·13	2·65
10	4·3177	0·46	0·48	2·66	2·3229	0·23	0·26	2·65
15	6·4271	0·69	0·72	2·65	3·4114	0·34	0·37	2·66
20	8·4687	0·90	0·96	2·65	4·4896	0·45	0·51	2·68
25	10·547	1·12	1·18	2·66	5·5208	0·56	0·63	2·62
30	12·583	1·34	1·40	2·69	6·5417	0·66	0·75	2·62
1	2	3	4	5	6	7	8	9

TABLE VII.

Model 7.

Weight of water displaced 215·97 ounces.

Exp. 1. Lever 27·08 inches. Centre of gravity 2·4 inches.

Exp. 2. Lever 25·78 inches. Centre of gravity 3·7 inches.

	Exp. 1.				Exp. 2.			
Deg.	Oz.	E R	Theor.	M C	Oz.	Gr.	Theor.	M C
5	2·1021	0·26	0·27	3·02	1·2708	0·21	0·16	3·04
10	4·2375	0·53	0·55	3·06	2·5835	0·31	0·32	3·07
15	6·4666	0·81	0·84	3·13	3·9687	0·47	0·50	3·13
20	8·8646	1·11	1·14	3·25	5·5469	0·66	0·69	3·24
25	11·588	1·45	1·46	3·44	7·4427	0·89	0·91	3·40
30	14·568	1·82	1·83	3·65	9·7864	1·17	1·17	3·64
1	2	3	4	5	6	7	8	9

TABLE VIII.

18 Gun Brig.

Weight of water displaced 186·96 ounces.

Exp. 1. Lever 25·35 inches. Centre of gravity 2·24 inches.

Exp. 2. Lever 24·05 inches. Centre of gravity 3·54 inches.

	Exp. 1.				Exp. 2.			
Deg.	Oz.	E R	Theor.	M C	Oz.	Gr.	Theor.	M C
5	2·1146	0·29		3·29	1·3626	0·19		3·31
10	4·1979	0·57		3·28	2·6667	0·34		3·27
15	6·1893	0·84		3·24	3·9271	0·50		3·25
20	8·0521	1·09		3·19	5·0417	0·65		3·20
25	9·8958	1·34		3·17	6·0989	0·78		3·17
30	11·695	1·58		3·15	7·1146	0·91		3·13
1	2	3	Mean	3·22	6	7	Mean	3·22
			4	5			8	9

TABLE IX.

Leopard of 50 guns.

Weight of water displaced 271·71 ounces.

Exp. 1. Lever 26·15 inches. Centre of gravity 2·38 inches.

Exp. 2. Lever 24·85 inches. Centre of gravity 3·68 inches.

	Exp. 1.				Exp. 2.			
Deg.	Oz.	E R	Theor.	M C	Oz.	Gr.	Theor.	M C
5	2·1562	0·21		2·38	1·0260	0·09		2·38
10	4·2500	0 41		2·36	2·0000	0·18		2·35
15	6·3750	0·61		2·37	3·0447	0·28		2·37
20	8·4583	0·81		2·38	4·0573	0·37		2·38
25	10·536	1·01		2·40	5·0573	0·46		2·39
30	12·510	1·20		2·41	5·9375	0·54		2·39
1	2	3	Mean	2·38	6	7	Mean	2·38
			4	5			8	9

TABLE X.

Howe of 120 guns.
Weight of water displaced 295·97 ounces.

	Exp. 1. Lever 26·18 inches. Centre of gravity 2·43 inches.				Exp. 2. Lever 24·63 Inches. Centre of gravity 3·43 inches.			
Deg.	Oz.	E R	Theor.	M C	Oz.	Gr.	Theor.	M C
5	2·1458	0·20		2·25	0·9635	0·08		2·26
10	4·2760	0·39		2·25	1·9062	0·16		2·25
15	6·3802	0·58		2·25	2·8281	0·24		2·25
20	8·4062	0·77		2·24	3·7552	0·33		2·25
25	10·495	0·96		2·27	4·6458	0·40		2·25
30	12·500	1·15		2·29	5·5669	0·48		2·27
1	2	3	Mean	2·26	6	7	Mean	2·26
			4	5			8	9

ARTICLE II.

On the Liquefaction of Chlorine and other Gases. By M. Faraday, Chemical Assistant in the Royal Institution.*

I TOOK advantage of the late cold weather (Feb. and March, 1823), to procure crystals of hydrate of chlorine for the purpose of analysis. The results are contained in a short paper in the Quarterly Journal of Science, vol. xv. Its composition is very nearly 27·7 chlorine, 72·3 water, or 1 proportional of chlorine, and 10 of water.

The President of the Royal Society having honoured me by looking at these conclusions, suggested, that an exposure of the substance to heat under pressure, would probably lead to interesting results: the following experiments were commenced at his request. Some hydrate of chlorine was prepared, and being dried as well as could be by pressure in bibulous paper, was introduced into a sealed glass tube, the upper end of which was then hermetically closed. Being placed in water at 60°, it underwent no change; but when put into water at 100°, the substance fused, the tube became filled with a bright yellow atmosphere, and, on examination, was found to contain two fluid substances: the one, about three-fourths of the whole, was of a faint yellow colour, having very much the appearance of water; the remaining fourth was a heavy bright yellow fluid, lying at the bottom of the former, without any apparent tendency to mix with it. As the tube cooled, the yellow atmosphere condensed into more of the yellow fluid, which floated in a film on the pale fluid, looking very like chloride of nitrogen; and at 70° the pale portion congealed, although even at 32° the yellow portion did not solidify. Heated up to 100° the yellow fluid appeared to boil, and again produced the bright coloured atmosphere.

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* Abstracted from two papers in the Phil. Trans. for 1823, Part II.

By putting the hydrate into a bent tube, afterwards hermetically sealed, I found it easy, after decomposing it by a heat of 100°, to distil the yellow fluid to one end of the tube, and so separate it from the remaining portion. In this way a more complete decomposition of the hydrate was effected, and when the whole was allowed to cool, neither of the fluids solidified at temperatures above 34°, and the yellow portion not even at 0°. When the two were mixed together, they gradually combined at temperatures below 60°, and formed the same solid substances as that first introduced. If, when the fluids were separated, the tube was cut in the middle, the parts flew asunder as if with an explosion, the whole of the yellow portion disappeared, and there was a powerful atmosphere of chlorine produced; the pale portion on the contrary remained, and when examined, proved to be a weak solution of chlorine in water, with a little muriatic acid, probably from the impurity of the hydrate used. When that end of the tube in which the yellow fluid lay was broken under a jar of water, there was an immediate production of chlorine gas.

I at first thought that muriatic acid and euchlorine had been formed; then, that two new hydrates of chlorine had been produced; but at last I suspected that the chlorine had been entirely separated from the water by the heat, and condensed into a dry fluid by the mere pressure of its own abundant vapour. If that were true, it followed, that chlorine gas, when compressed, should be condensed into the same fluid, and, as the atmosphere in the tube in which the fluid lay was not very yellow at 50° or 60°, it seemed probable that the pressure required was not beyond what could readily be obtained by a condensing syringe. A long tube was therefore furnished with a cap and stop-cock, then exhausted of air and filled with chlorine, and being held vertically with the syringe upwards, air was forced in, which thrust the chlorine to the bottom of the tube, and gave a pressure of about four atmospheres. Being now cooled, there was an immediate deposit in films, which appeared to be hydrate, formed by water contained in the gas and vessels, but some of the yellow fluid was also produced. As this however might also contain a portion of the water present, a perfectly dry tube and apparatus were taken, and the chlorine left for some time over a bath of sulphuric acid before it was introduced. Upon throwing in air and giving pressure, there was now no solid film formed, but the clear yellow fluid was deposited, and more abundantly still upon cooling. After remaining some time it disappeared, having gradually mixed with the atmosphere above it, but every repetition of the experiment produced the same results.

Presuming that I had now a right to consider the yellow fluid as pure chlorine in the liquid state, I proceeded to examine its properties, as well as I could when obtained by heat from the hydrate. However obtained, it always appears very limpid and fluid, and excessively volatile at common pressure. A portion was cooled in its tube to 0°; it remained fluid. The tube was then opened, when a part immediately flew off, leaving the rest so cooled by the evaporation as to remain a fluid under the atmospheric pressure. The temperature could not have been higher than −40° in this case; as Sir Humphry Davy has shown that dry chlorine does not condense at that temperature under common pressure. Another tube was opened at a temperature of 50°; a part of the chlorine volatilised, and cooled the tube so much as to condense the atmospheric vapour on it as ice.

A tube having the water at one end and the chlorine at the other was weighed, and then cut in two; the chlorine immediately flew off, and the loss being ascertained was found to be 1·6 grain: the water left was examined and found to contain some chlorine: its weight was ascertained to be 5·4 grains. These proportions, however, must not be considered as indicative of the true composition of hydrate of chlorine; for, from the mildness of the weather during the time when these experiments were made, it was impossible to collect the crystals of hydrate, press, and transfer them, without losing much chlorine; and it is also impossible to separate the chlorine and water in the tube perfectly, or keep them separate, as the atmosphere within will combine with the water, and gradually reform the hydrate.

Before cutting the tube, another tube had been prepared exactly like it in form and size, and a portion of water introduced into it, as near as the eye could judge, of the same bulk as the fluid chlorine: this water was found to weigh 1·2 grain; a result, which, if it may be trusted, would give the specific gravity of fluid chlorine as 1·33; and from its appearance in, and on water, this cannot be far wrong.

The refractive power of fluid chlorine is rather less than that of water. The pressure of its vapour at 60° is nearly equal to four atmospheres.

[*Note on the Condensation of Muriatic Acid Gas into the liquid Form.* By Sir H. Davy, Bart. Pres. R.S.

In desiring Mr. Faraday to expose the hydrate of chlorine to heat in a closed glass tube, it occurred to me, that one of three things would happen; that it would become fluid as a hydrate; or that a decomposition of water would occur, and euchlorine and muriatic acid be formed; or that the chlorine would separate in a condensed state. This last result having been obtained, it evidently led to other researches of the same kind. I shall hope, on a future occasion, to detail some general views on the subject of these researches. I shall now merely mention, that by sealing muriate of ammonia and sulphuric acid in a strong glass tube, and causing them to act upon each other, I have procured liquid muriatic acid: and by substituting carbonate for muriate of ammonia, I have no doubt that carbonic acid may be obtained, though in the only trial I have made the tube burst. I have requested Mr. Faraday to pursue these experiments, and to extend them to all the gases which are of considerable density, or to any extent soluble in water; and I hope soon to be able to lay an account of his results, with some applications of them that I propose to make, before the Society.

I cannot conclude this note without observing, that the generation of elastic substances in close vessels, either with or without heat, offers much more powerful means of approximating their molecules than those dependent upon the application of cold, whether natural or artificial: for, as gases diminish only about 1-480 in volume for every — degree of Fahrenheit's scale, beginning at ordinary temperatures, a very slight condensation only can be produced by the most powerful freezing mixtures, not half as much as would result from the application of a strong flame to one part of a glass tube, the other part being of ordinary temperature; and when attempts are made to condense gases into fluids by sudden mechanical compression, the heat, instantly generated, presents a formidable obstacle to the success of the experiment; whereas, in the compression resulting from their slow generation in close vessels, if the process be conducted with common precautions, there is no source of difficulty or danger; and it may be easily assisted by artificial cold in cases when gases approach near to that point of compression and temperature at which they become vapours.]

The refractive power of liquid muriatic acid is greater than that of nitrous oxide, but less than that of water; it is nearly equal to that of carbonic acid. The pressure of its vapour at the temperature of 50° is equal to about 40 atmospheres.

Sulphurous Acid.

Mercury and concentrated sulphuric acid were sealed up in a bent tube, and, being brought to one end, heat was carefully applied, while the other end was preserved cool by wet bibulous paper. Sulphurous acid gas was produced where the heat acted, and was condensed by the sulphuric acid above; but, when the latter had become saturated, the sulphurous acid passed to the cold end of the tube, and was condensed into a liquid. When the whole tube was cold, if the sulphurous acid were returned on to the mixture of sulphuric acid and sulphate of mercury, a portion was re-absorbed, but the rest remained on it without mixing.

Liquid sulphurous acid is very limpid and colourless, and highly fluid. Its refractive power, obtained by comparing it in water and other media, with water contained in a similar tube, appeared to be nearly equal to that of water. It does not solidify or become adhesive at a temperature of 0° F. When a tube containing it was opened, the contents did not rush out as with explosion, but a portion of the liquid evaporated rapidly, cooling another portion so much as to leave it in the fluid state at common barometric pressure. It was however rapidly dissipated, not producing visible fumes, but producing the odour of pure sulphurous acid, and leaving the tube quite dry. A portion of the vapour of the fluid received over a mercurial bath, and examined, proved to be sulphurous acid gas. A piece of ice dropped into the fluid instantly made it boil, from the heat communicated by it.

To prove in an unexceptionable manner that the fluid was pure sulphurous acid, some sulphurous acid gas was carefully prepared over mercury, and a long tube perfectly dry, and closed at one end, being exhausted, was filled with it; more sulphurous acid was then thrown in by a condensing syringe, till there were three or four atmospheres; the tube remained perfectly clear and dry, but on cooling one end to 0°, the fluid sulphurous acid condensed, and in all its characters was like that prepared by the former process.

A small gage was attached to a tube in which sulphurous acid was afterwards formed, and at a temperature of 45° F. the pressure within the tube was equal to three atmospheres, there being a portion of liquid sulphurous acid present: but as the common air had not been excluded when the tube was sealed, nearly one atmosphere must be due to its presence, so that sulphurous acid vapour exerts a pressure of about two atmospheres at 45° F. Its specific gravity was nearly 1·42.*

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* I am indebted to Mr. Davies Gilbert, who examined with much attention the results of these experiments, for the suggestion of the means adopted to obtain the specific gravity of some of these fluids. A number of small glass bulbs were blown and hermetically sealed; they were then thrown into alcohol, water, sulphuric acid, or mix-

Sulphuretted Hydrogen.

A tube being bent, and sealed at the shorter end, strong muriatic acid was poured in through a small funnel, so as nearly to fill the short leg without soiling the long one. A piece of platinum foil was then crumpled up and pushed in, and upon that were put fragments of sulphuret of iron, until the tube was nearly full. In this way action was prevented until the tube was sealed. If it once commences, it is almost impossible to close the tube in a manner sufficiently strong, because of the pressing out of the gas. When closed, the muriatic acid was made to run on to the sulphuret of iron, and then left for a day or two. At the end of that time, much protomuriate of iron had formed, and on placing the clean end of the tube in a mixture of ice and salt, warming the other end if necessary by a little water, sulphuretted hydrogen in the liquid state distilled over.

The liquid sulphuretted hydrogen was colourless, limpid, and excessively fluid. Ether, when compared with it in similar tubes, appeared tenacious and oily. It did not mix with the rest of the fluid in the tube, which was no doubt saturated, but remained standing on it. When a tube containing it was opened, the liquid immediately rushed into vapour; and this being done under water, and the vapour collected and examined, it proved to be sulphuretted hydrogen gas. As the temperature of a tube containing some of it rose from 0° to 45°, part of the fluid rose in vapour, and its bulk diminished; but there was no other change: it did not seem more adhesive at 0° than at 45°. Its refractive power appeared to be rather greater than that of water; it decidedly surpassed that of sulphurous acid. A small gage being introduced into a tube in which liquid sulphuretted hydrogen was afterwards produced, it was found that the pressure of its vapour was nearly equal to 17 atmospheres at the temperature of 50°.

The gages used were made by drawing out some tubes at the blowpipe table until they were capillary, and of a trumpet form; they were graduated by bringing a small portion of mercury successively into their different parts; they were then sealed at the fine end, and a portion of mercury placed in the broad end; and in this state they were placed in the tubes, so that none of the substances used, or produced, could get to the mercury, or

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tures of these, and when any one was found of the same specific gravity as the fluid in which it was immersed, the specific gravity of the fluid was taken: thus a number of hydrometrical bulbs were obtained; these were introduced into the tubes in which the substances were to be liberated; and ultimately, the dry liquids obtained, in contact with them. It was then observed whether they floated or not, and a second set of experiments were made with bulbs lighter or heavier as required, until a near approximation was obtained. Many of the tubes burst in the experiments, and in others difficulties occurred from the accidental fouling of the bulb by the contents of the tube. One source of error may be mentioned in addition to those which are obvious, namely, the alteration of the bulk of the bulb by its submission to the pressure required to keep the substance in the fluid state.

pass by it to the inside of the gage. In estimating the number of atmospheres, one has always been subtracted for the air left in the tube.

The specific gravity of sulphuretted hydrogen appeared to be 0·9.

Carbonic Acid.

The materials used in the production of carbonic acid, were carbonate of ammonia and concentrated sulphuric acid; the manipulation was like that described for sulphuretted hydrogen. Much stronger tubes are however required for carbonic acid than for any of the former substances, and there is none which has produced so many or more powerful explosions. Tubes which have held fluid carbonic acid well for two or three weeks together, have, upon some increase in the warmth of the weather, spontaneously exploded with great violence; and the precautions of glass masks, goggles, &c. which are at all times necessary in pursuing these experiments, are particularly so with carbonic acid.

Carbonic acid is a limpid colourless body, extremely fluid, and floating upon the other contents of the tube. It distils readily and rapidly at the difference of temperature between 32° and 0°. Its refractive power is much less than that of water. No diminution of temperature to which I have been able to submit it, has altered its appearance. In endeavouring to open the tubes at one end, they have uniformly burst into fragments, with powerful explosions. By inclosing a gage in a tube in which fluid carbonic acid was afterwards produced, it was found that its vapour exerted a pressure of 36 atmospheres at a temperature of 32°.

Euchlorine.

Fluid euchlorine was obtained by inclosing chlorate of potash and sulphuric acid in a tube, and leaving them to act on each other for 24 hours. In that time there had been much action, the mixture was of a dark reddish brown, and the atmosphere of a bright yellow colour. The mixture was then heated up to 100°, and the unoccupied end of the tube cooled to 0°; by degrees the mixture lost its dark colour, and a very fluid ethereal looking substance condensed. It was not miscible with a small portion of the sulphuric acid which lay beneath it; but when returned on to the mass of salt and acid, it was gradually absorbed, rendering the mixture of a much deeper colour even than itself.

Euchlorine thus obtained is a very fluid transparent substance, of a deep yellow colour. A tube containing a portion of it in the clean end, was opened at the opposite extremity; there was a rush of euchlorine vapour, but the salt plugged up the aperture: while clearing this away, the whole tube burst with a violent explosion, except the small end in a cloth in my hand, where the euchlorine previously lay, but the fluid had all disappeared.

Nitrous Oxide.

Some nitrate of ammonia, previously made as dry as could be by partial decomposition, by heat in the air, was sealed up in a bent tube, and then heated in one end, the other being preserved cool. By repeating the distillation once or twice in this way, it was found, on after-examination, that very little of the salt remained undecomposed. The process requires care. I have had many explosions occur with very strong tubes, and at considerable risk.

When the tube is cooled, it is found to contain two fluids, and a very compressed atmosphere. The heavier fluid on examination proved to be water, with a little acid and nitrous oxide in solution; the other was nitrous oxide. It appears in a very liquid, limpid, colourless state; and so volatile that the warmth of the hand generally makes it disappear in vapour. The application of ice and salt condenses abundance of it into the liquid state again. It boils readily by the difference of temperature between 50° and 0°. It does not appear to have any tendency to solidify at – 10°. Its refractive power is very much less than that of water, and less than any fluid that has yet been obtained in these experiments, or than any known fluid. A tube being opened in the air, the nitrous oxide immediately burst into vapour. Another tube opened under water, and the vapour collected and examined, it proved to be nitrous oxide gas. A gage being introduced into a tube, in which liquid nitrous oxide was afterwards produced, gave the pressure of its vapour as equal to above 50 atmospheres at 45°.

Cyanogen.

Some pure cyanuret of mercury was heated until perfectly dry. A portion was then inclosed in a green glass tube, in the same manner as in former instances, and being collected to one end, was decomposed by heat, while the other end was cooled. The cyanogen soon appeared as a liquid: it was limpid, colourless, and very fluid; not altering its state at the temperature of 0°. Its refractive power is rather less, perhaps, than that of water. A tube containing it being opened in the air, the expansion within did not appear to be very great; and the liquid passed with comparative slowness into the state of vapour, producing great cold. The vapour, being collected over mercury, proved to be pure cyanogen.

A tube was sealed up with cyanuret of mercury at one end, and a drop of water at the other; the fluid cyanogen was then produced in contact with the water. It did not mix, at least in any considerable quantity, with that fluid, but floated on it, being lighter, though apparently not so much so as ether would be. In the course of some days, action had taken place, the water had become black, and changes, probably such as are known to take place in an aqueous solution of cyanogen, occurred. The pressure of the vapour of cyanogen appeared by the gage to be 3·6 or 3·7 atmospheres at 45°. Its specific gravity was nearly 0·9.

Ammonia.

In searching after liquid ammonia, it became necessary, though difficult, to find some dry source of that substance; and I at last resorted to a compound of it, which I had occasion to notice some years since with chloride of silver.* When dry chloride of silver is put into ammoniacal gas, as dry as it can be made, it absorbs a large quantity of it: 100 grains condensing above 130 cubical inches of the gas; but the compound thus formed is decomposed by a temperature of 100° F. or upwards. A portion of this compound was sealed up in a bent tube and heated in one leg, while the other was cooled by ice or water. The compound thus heated under pressure fused at a comparatively low temperature, and boiled up, giving off ammoniacal gas, which condensed at the opposite end into a liquid.

Liquid ammonia thus obtained was colourless, transparent, and very fluid. Its refractive power surpassed that of any other of the fluids described, and that also of water itself. From the way in which it was obtained, it was evidently as free from water as ammonia in any state could be. When the chloride of silver is allowed to cool, the ammonia immediately returns to it, combining with it, and producing the original compound. During this action a curious combination of effects takes place: as the chloride absorbs the ammonia, heat is produced, the temperature rising up nearly to 100°; while a few inches off, at the opposite end of the tube, considerable cold is produced by the evaporation of the fluid. When the whole is retained at the temperature of 60°, the ammonia boils till it is dissipated and re-combined. The pressure of the vapour of ammonia is equal to about 6·5 atmospheres at 50°. Its specific gravity was 0·76.

Attempts have been made to obtain hydrogen, oxygen, fluoboracic, fluosilicic, and phosphuretted hydrogen gases in the liquid state; but though all of them have been subjected to great pressure, they have as yet resisted condensation.

ARTICLE III.

New Locality of the Skorodite. By W. Phillips, FLS. MGS. &c.

THE crystals forming the subject of this notice were lately received in a letter addressed to me from "Calenick, near Truro," and signed "J. Michell," as having been "obtained from a mine in the neighbourhood of St. Austell," in Cornwall. By the gentleman who transmitted them, they are imagined to be a variety of the arseniate of iron; but he laments that their scarcity had prevented his ascertaining their composition, and requests the insertion of a notice respecting them in the *Annals of Philosophy*.

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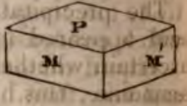
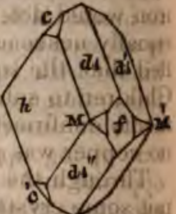
* Quarterly Journal of Science, vol. v. p. 74.

The largest of these crystals does not exceed in size the head of an ordinary pin, but many of them are so complete as to leave it a matter of doubt whether they ever were attached to a matrix; a few, however, are deposited on some small fragments of quartz. In form they very closely resemble that of the skorodite, given in the third edition of my Elementary Introduction to Mineralogy; the planes d_2 and d_2', are, however, wanting in the second of the following figures, which represents the form of the crystals lately received from Cornwall; while almost every one of them exhibits the planes *c c*, which are not observable in the crystals of the skorodite, or in those of the martial arseniate of copper.





Externally these crystals are of the dark bottle-green colour, very common to some of the prismatic varieties of the arseniate of copper; but this is not in fact the true colour of the substance itself, which, on holding the crystals, or thin fragments of them between the eye and the light, is found by the assistance of a glass to be of the pale blue, so common to the martial arseniate of copper. The dark-green colour arises from the mechanical intermixture of a multitude of very minute specks, of that colour, visible on the surface, and also by transmitted light.

M on M	120°	2′
M on *h*	119	55
M on d_1	141	0
d_1 on d_1'	103	45
d_1 on d_1''	112	36
c on *h*	154	20

The first figure represents a right rhombic prism, the primary form of the martial arseniate of copper, the skorodite, and also of the crystals which form the subject of this notice: the planes d_1, d_1, of the latter, generally present several reflections less than one degree apart, indicating each to be a series of planes.

The foregoing measurements by the reflective goniometer, as well as the form of these crystals, tend to show that they are only a variety of the martial arseniate of copper, which commonly is prismatic, the planes of the prism being the primary planes M M′, sometimes associated with the planes *f* and *h*, and the prism is commonly terminated by one quadrangular pyramid formed by the planes *d d′*; but in these crystals, and also in the skorodite, the planes M M′ are reduced to small triangles, owing to the presence of both pyramids.

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* Quarterly Journal of Science, vol. v. p. 74.

Supposing that the iron and copper exist in the mineral in the state of peroxide, and that the weights of their atoms are to each other respectively as 40 to 80, it will be impossible to reduce the martial arseniate of copper to a probable definite compound; for it will appear by calculation that the nearest approximation is 5 atoms of oxide of iron and 2 atoms of oxide of copper; it seems, therefore, more likely that the skorodite is a peculiar arseniate of iron, differing not only in form, but in composition, from the cubic arseniate of iron; and it will follow, if this be admitted, that the martial arseniate of copper is a mixture and not a compound of arseniate of iron and arseniate of copper: this supposition will, perhaps, be considered the more probable when it is remembered that the cubic arseniate of iron contains 9 per cent. of oxide of copper. It is also to be observed, that M. Chenevix inclines to the opinion that it is a mixture of the two arseniates; and lastly, there appears to be no reason why there should not exist several varieties of arseniate of iron, which is well known to be the case with arseniate of copper.

Article IV.

On some Compounds of Fluorine. By J. Smithson, Esq. FRS.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Jan.* 2, 1824.

When numberless persons are seen, in every direction, pursuing a subject with the utmost ardour, it is natural to conclude that their labours have accomplished all that was within their reach to perform.

It must, therefore, in mineralogy be supposed, that those substances whose abundance has placed them in every hand, have been fully scrutinized, and are thoroughly understood; and that if now to extend the boundaries of the science it is not indispensable to explore new regions of the earth, and procure matters hitherto unpossessed, it is yet only to objects the most rare, the most difficult of acquisition, that inquiry can be applied with any hope of new results.

A want of due conviction that the materials of the globe and the products of the laboratory are the same, that what nature affords spontaneously to men, and what the art of the chemist prepares, differ no ways but in the sources from whence they are derived, has given to the industry of the collector of mineral bodies an erroneous direction.

What is essential to a knowledge of chemical beings has been left in neglect; accidents of small import, often of none, have fixed attention—have engrossed it; and a fertile field of discovery has thus remained where otherwise it would have been exhausted.

Fluor spar has decorated mineral cabinets from probably the earliest period of their existence; every tint with which chance can paint it; each casual diversity of form and appearance under which it may present itself have been long familiar, and its true nature continues a problem; and its decomposition by fire was yet to be learned.

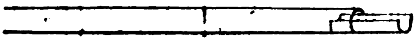
Fluor Spar.

If a very minute fragment of fluor spar is fastened by means of clay* to the end of a platina wire nearly as fine as a hair, which is the size I now employ even with fluxes, it will be perceived on the first contact of the fire to melt with great facility. As the fusion is prolonged, the fusibility will decrease; protuberances will rise over the surface of the ball; it will put on what is designated by the term of the cauliflower form; and finally become entirely refractory. On detaching it from the wire, it will prove hollow. This little capsula being taken up again by its side, and its edge presented to the flame, thin and porous as this edge is, it will withstand its utmost violence.

Such an alteration of qualities proclaims an equal one of nature. I had no doubt that the calcium had absorbed oxygen, and parted with fluorine; that the mass had ceased to be fluor spar, and was become quicklime. On placing it in a drop of water my conjecture was confirmed; a solution took place by which test papers were altered; a *cremor calcis* soon appeared; and on allowing the mixture to become spontaneously dry, a white powder remained, which acids dissolved with effervescence.

That the fluoric element was gone admitted not of doubt. To pursue it in its escape; to coerce it, and render it palpable to the senses, could not be required to establish the fact. It may, however, be done.

The open tube described by M. Berzelius in his valuable work on the blowpipe, is adapted to the purpose by an addition to it. A small plate of platina foil, on a curved plate of baked clay, is introduced a little way into one of its ends; and secured by bringing with the point of the flame the glass into contact with it. The body to be tried is fixed to this plate by means of moist clay; and may then be subjected for any time to any degree of heat.



Thus tried, fluor spar quickly obscured the glass by a thick crust of siliceous matter; and coloured yellow a bit of paper tinged with logwood.

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* *Annals* for December.

M. Berzelius assigns fernambuc wood for the test of fluoric acid. Bergman says that this wood affords a red infusion which alkalies turn blue.* None such could be procured; but it was found that logwood might be substituted for it. The paper tinged with this, like that mentioned by M. Berzelius, is made yellow by fluoric acid and oxalic acid; but it did not seem to be so by sulphuric or muriatic acids, nor by phosphoric acid.

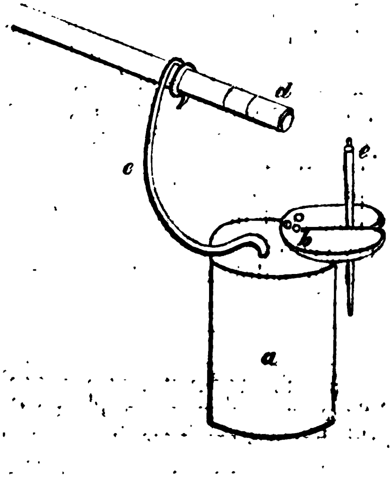
Topaz.

In extremely minute particles, topaz subjected to the fire at the end of a very slender wire soon becomes opaque and white; but I perceived no marks of fusion.

This change is undoubtedly occasioned by the loss of its fluoric part. One of the times I was at Berlin, M. Klaproth gave me, as his reason for not publishing the analysis of topaz, that in the porcelain furnace it sustained a great loss of weight, the cause of which he had not then been able to ascertain.

Topaz ground to impalpable powder, and blended with carbonate of lime, melted with ease. Some of this mixture fused on the platina plate at the mouth of the tube, made an abundant deposit of silica over its interior surface; and the bit of logwood paper at the end of it had its blue colour altered to yellow.

In the trial in this way of substances of difficult fusion, an apparatus of the following construction is more favourable than the one above described.



a. A bottle cork.
b. A slice of the same fixed with three pins.
c. A wire.

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* Analysis of Mineral Waters.

d. A cylinder of platina foil introduced into the mouth of the glass tube, to prevent its being softened and closed by the flame.

e. A platina wire, at the end of which is cemented with clay the subject of trial.

I formerly suggested that topaz might be a compound of silicate of alumina, and of fluate of alumina.* I am now convinced that no oxygen exists in it; but that it is a combination of the fluorides of silicium and aluminum.

This system produces a considerable alteration in the proportions of its elements.

The mean of the six analyses quoted by M. Haüy, in the second edition of his Mineralogy, is

Silica	36·0
Alumina	52·8
Fluoric acid	9·7
	98·0

Deducting the oxygen from the metals, we have

Silicium	18·0
Aluminium	27·7
Fluorine	52·3
	98·0

Kryolite.

It has been observed to diminish in fusibility during fusion,† and it was in every respect probable, from what had been seen with the foregoing bodies, that it would be decomposed in the fire. After being kept some time melted, it afforded an alkaline solution, which, by exposure to the air, became carbonate of soda, effloresced, effervesced with nitric acid, and produced crystals of nitrate of soda.

Fused on the platina plate at the mouth of the tube, a copious deposit of silex collected in the tube; and the bit of logwood paper became very yellow.

Kryolite heated in sulphuric acid on glass destroyed its polish.

1. These experiments render it highly probable that fluorine will be expelled from every compound of it by the agency of fire; and consequently that we are now in possession of a general method of discovering its presence in bodies. In cases where a matter is infusible, and parts with it with great difficulty, as in

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* Philosophical Transactions for 1811.

† Haüy's Mineralogy.

that of topaz, it may be required to reduce it to fine powder, or to act upon it by some admixture with which it melts, for the sake of promoting division, and multiplying surfaces.

Hereby is supplied what may have seemed to be an omission in the paper on acids.* Although it was not such, since fluorine is not an acid; and fluoric acid may never occur in a mineral substance; as it can probably exist in combination only with ammonia; all its other supposed compounds being doubtless fluorides.

2. The theory of these decompositions may be acquired by experiment; and light obtained on the nature of the compounds.

If fluor spar, for instance, is a combination of oxide of calcium and fluoric acid, and this is expelled from the oxide merely by the force of fire, the decomposition of it will take place in closed vessels without the presence of oxygen or of water; fluoric acid will be obtained; and the weight of this acid and the lime will be equal together to that of the original spar.

If the spar is metallic calcium and fluorine, and when heated in oxygen absorbs this, and parts with fluorine, it is fluorine which will be collected in the vessels, and its weight and that of the lime will together exceed that of the spar by the oxygen of the lime.

If it is water which is the agent of decomposition, fluoric acid will be collected; but here the excess of weight will not only equal the oxygen absorbed by the lime, but also the hydrogen which has acidified the fluorine; and this increased weight of the fluoric acid will prove that hydrogen is an element of it.

It appears to have been fluoric acid which in the above related experiments passed into the tubes; but the inflammable matter of the flame would probably have rendered emitted fluorine such. It becomes of high importance to ascertain whether ignited fluor spar is decomposed by passing water over it, and if so what are the products. It is not convenient to myself at present to make the experiment: I therefore resign it to others.

How far the difficulty which the action of fluorine on the vessels in which it is contained, as opposed to its examination, would be obviated by employing vessels of its compounds, as of fluor spar, or of chloride of silver; or whether it acts on all oxides as it does on silica, experiments have not informed me.

3. The vegetation of matters before the blowpipe is attributed by a great chemist to "a new state of equilibrium induced by heat between the constituent parts of bodies,"† but the phenomena do not accord with the explanation.

Was such the cause of the acquired infusibility, it would manifest itself through the whole mass as soon as fusion had enabled the new arrangement. It is, on the contrary, confined to the surface; the interior portion continues fluid; but where-ever any of this bursts the shell, and issues forth, it is instantly fixed in immovable solidity; and when the process has attained its final state, a hollow globule remains.

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* Annals for May.

† De l'Emploi du Chalumeau, p. 98.

Why is the change of quality limited to the surface; how has been produced the central cavity; what has forced away the matter which occupied it? A new element has been received from without, one which existed in the matter has been parted with in a state of vapour. This double action may probably be inferred wherever a matter presents this species of vegetation.

Some metallic bodies, as tin, lead, sulphuretted tin, arsenicated nickel, &c. present another species of vegetation, caused by the absorption of oxygen, and the production over their surface of a matter more bulky than the metal from which it is produced, and infusible at the heat to which it is exposed. Here no internal void forms.

The mode of fusion of epidote had led me to suspect the existence of fluorine in it; but on trial with the second apparatus, represented above, I could not perceive a trace of it. A more accurate observation of its fusion has shown me that it does not, as generally supposed, form the cauliflower. It appears to do so only where so large a mass is exposed to the fire that but points of its surface are fused in succession. If a very minute bit is employed, it is clearly seen to puff up like borax, stilbite, &c.; and then, like them, become less fusible; from the separation, doubtless, of a vapourized element on which its greater fusibility had depended. The smallest particle of fluor spar shows no such inflation.

We see here three several cases of intumescence in the fire; one where a gas is absorbed; one where a gas, or vapour, is disengaged; one where the two effects are concomitant.

There may be persons who, measuring the importance of the subject by the magnitude of the objects, will cast a supercilious look on this discussion; but the particle and the planet are subject to the same laws; and what is learned upon the one will be known of the other.

Article V.

On the Composition of the Ancient Ruby Glass. By Mr. J. T. Cooper.

(To the Editor of the *Annals of Philosophy.*)

SIR, Jan. 11, 1824.

The chief difference between the ancient and modern ruby glass, I have understood from those who are in the habit of using the latter in large quantities, consists in the hardness, or infusibility of the basis on which it is flashed, that which is now manufactured being of flint, while the former is of the hardest crown glass; also the difficulty of obtaining it of any size, and free from cloudiness or opacity: to ascertain the composition of the ancient glass, I made the following experiments.

A quantity of the glass was sent me by Mr. Charles Muss, and such pieces were selected for examination as were free from decomposition, and of the deepest colour: these were powdered in a stone mortar, and afterwards mixed with four times their weight of carbonate of potash; the mixture was heated to fusion in a hessian crucible, and the fused mass poured out while fluid. This was afterwards powdered and digested in muriatic acid, which dissolved nearly the whole, what remained appearing to be mostly silex. The acid solution was slowly evaporated nearly to dryness, and distilled water poured on the mass, to wash it. To the filtered solution ammonia was added in excess, which threw down an abundant precipitate of oxide of iron, the supernatant fluid acquiring a deep blue tinge, which, upon examination, proved to contain only copper. The filter that contained the silex stood for some hours near a window, and the surface of the silex gradually assumed a deeper colour, approximating at last to a deep brown. Suspecting it to contain muriate of silver, I washed it with a solution of ammonia. On adding muriatic acid to the filtered solution, a copious precipitate of chloride of silver ensued.

The precipitate of iron, and the ammoniacal solution containing the copper, were carefully examined for other substances, and particularly for manganese, which I know has been suspected to enter into the composition of this coloured glass, but I was not able to detect the smallest portion. The only substance I found, except those I have mentioned, was a slight trace of lime. From the above, it is evident the composition of this glass may be stated to be

Silex,
Oxide of copper,
Oxide of iron,
Oxide of silver,
Lime.

It is difficult to decide whether the oxide of iron enters into the composition of the coloured portion of the glass, or into the bases or substance of it, or both. I detached some small fragments of the uncoloured portion, and made a separate examination of them, and they proved to contain abundance of iron. It is also difficult to determine what alkali has been used as a flux for the siliceous matter. The quantity of lime I obtained was certainly much too small to produce the effect; but I have some reason to suspect the alkali to be soda.

To endeavour to determine the exact proportions of the above colouring ingredients, which I consider to be the oxide of copper and the oxide of silver, would be useless. The colouring matter which forms only a film of at most 1-200th of an inch in thickness upon a substance of glass varying from 1-30th to 1-10th of an inch, is quite sufficient reason for desisting. I attempted some time since to grind the uncoloured portion away; and in another instance, to detach it by fluoric acid, but in each of these attempts I was unsuccessful. That class of your readers to whom this communication may be of any service, if not fully aware of the proportions of the colouring oxides, may easily obtain them by a few experiments.

Article VI.

On the ensuing Opposition of Mars. By F. Baily, Esq. FRS. VP. Ast. Soc. (Read before the Astronomical Society of London, Jan. 9, 1824.)*

At a time when we have two new and excellent observatories established in the *southern* hemisphere, where the celestial phænomena are watched and observed with the greatest diligence and zeal, it becomes the more important and necessary that corresponding observations of a certain class of those phænomena, of not very frequent occurrence, should also be made in the *northern* hemisphere, by such persons as are fortunately possessed of the requisite means for this purpose. Without this co-operation, the labours of those industrious observers will lose much of their value, and the advantageous opportunity of elucidating an important branch of physical astronomy will be wholly lost to the public.

The ensuing opposition of Mars, on the 24th of March, is one of this class: a phænomenon which occurs once only in a period of about 780 days. It is well known that corresponding observations of this planet, in the two hemispheres, as compared with stars situated near its path, about the period of its opposition, will serve to determine its parallax. And the parallax of Mars being known, that of the sun may thence be deduced. This was the plan adopted by Lacaille, when he was at the Cape of Good Hope, in the year 1751; since which period, the method has fallen into disuse for want of an observatory in the southern hemisphere, with instruments fit to be compared with those in Europe.

The present period seems extremely favourable (for the reasons above mentioned) for the revival of this method. At the time of the last opposition in 1822, I ventured to draw the public attention to the subject, by pointing out certain stars, near which the planet would pass; and with the positions of which it might be compared. Several valuable observations were made both in the southern and in the northern hemisphere, which are published in various periodical works; and which being thus recorded, may be referred to with advantage, by those who devote themselves to this branch of physical astronomy.

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* See our report of the proceedings of this Society in the present Number.—Edit.

At the present opposition, there are but few stars, and those of inferior magnitude, with which Mars can be advantageously compared. For ten days preceding and subsequent to the date of its opposition, Mars will not approach *near* to any star given in the large catalogues of Bradley or Piazzi. There are, however, five stars given in the catalogues of Lalande, inserted in the *Connaissance des Tems* for the years VIII. and XIII. with which the comparisons may be made. The mean places of these stars, on Jan. 1 of the present year, are given in the following little table; together with the dates when Mars will be in conjunction with them.

Con. des tems.	Mag.	AR.			D.			Mars.	
An. XIII.	7, 8	12h	10m	0s	2°	18′	44″N	April	1
XII.	8	0	14	29	1	52	2	March	29
XIII.	8	0	17	16	1	29	47		27
VIII.	7	0	20	8	1	8	37		25
XIII.	6, 7	0	29	56	0	6	44		19

When Mars approaches either of these stars, the observer should, with a micrometer, measure their distance, in a direct line, or take the differences, in right ascension and declination, between the planet and the star; the place and the correct time of observation being noted down.

Accurate observations of this kind are of great importance in astronomy; and as nothing tends so much to further such objects as a previous announcement of the phænomena about to take place, I trust I need not make any apology for drawing the attention of the members of this Society to so interesting a subject.

The diameter of Mars, on the day of opposition, will be 13·91″.

ARTICLE VII.

Table of the Salt Springs in Germany and the Neighbouring Countries. By C. Keferstein, of Halle, in Saxony.

(Communicated by the Author; and with his Synonymes of the Formations.)

	Names of the Springs.	Depth of the springs in German ft.*	Temperatures. Degrees.	Contents, per cent.	Present produce in lasts of 4000 lb.†	Formations in which the Springs occur.
	I. *In Prussia.*					
1	Colberg, in Pomerania	25	—	to 5¼	1500	Jungste sand formation (*London clay*).
2	Greifswalde, ditto	34	7—8	2½—3	300	Ditto.
3	Königsborn, near Unna, in Westphalia	49½	9	4 7/10	3023	Gruner Mergel; (*Tuffeau* of the French: *green sand—the series between the chalk and the oolites, in England.*)
4	Werl, in ditto	60	—	7½	900	Ditto.
5	Sassendorf, near Soest, ditto	—	—	—	640	
6	Westerkotten, ditto	—	—	3⅘	500	
7	Salzkotten, ditto	20	—	6 1/16	600	
8	Munster on the Stein, and,	210	—	1½	270	Porphyr-Steinkohlen formation (*great coal formation*).
9	Theodorshall, near Creuznach and Mayence				888	
10	Rheine, in Westphalia, near Munster	180	—	4 1/10	330	Formation des gryphiten kalkes (between the muschel and the jura kalk, & belonging to *oolt. series*).
11	Rölching, near Sargemines and Saarbrucken	40	—	2½	[illegible]	Ditto.
12	Neusalzwerk, near Rehme, and Minden, in Westphalia	66	—	11½	700	Ditto.
13	Halle, in Saxony	72	10	21 19 15	6300	Muschelkalk (*lias*).
14	Schönebeck, near Magdeburg	960	12	11½	15000	Between the muschelkalk (*lias*), and bunter sandstein—(*red marls*).
15	Stassfurth, ditto	167		17½	1300	Bunter sandstein (*new red sandstone*).
16	Koesen, near Naunburg	588	15	4	1340	Rother mergel (*red marls*).

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* "The Rhineland or Prussian foot, which is used by land-surveyors, in most parts of Germany, contains 12·36 English inches, or 0·3139 metres."—Kelly's Cambist.—*Edit.*

† "100 lb. of Prussia equal 103·11 lb. averdupois."—Ibid.

Names of the Springs.	Depth of the springs in German feet.	Temperatures. Degrees.	Contents, per cent.	Present produce in lasts of 4000 lbs.	Formations in which the Springs occur.
17 Durrenberg, ditto	712	14	8	6500	Bunter sandstein.
18 Teuditz, ditto	451	9	8	480	Ditto.
19 Kötschau	194	10½	3		
20 Artern, in Thuringia	—	10	3½	1450	Ditto.
II. *The Grand Duchy of Mecklenburg.*					
1 Sulze	45	9½	3¼ 5½	440	Sand formation (*London clay*).
III. *The Duchy of Holstein.*					
1 Oldesloe	—	—	1¼ 1½	1200	Ditto.
IV. *Saxony.*					
1 Frankenhausen, in Thuringia	64	—	11	2600	Zechstein (*newer magnesian limestone*).
2 Sulza, near Naunburg	190 350	—	—	800	Muschelkalk (*lias*).
3 Friedrichshall, near Lindenau and Hildburghausen	—	—	$2\frac{2}{10}$ $1\frac{4}{10}$	25	Ditto.
4 Salzungen, near Eisenach	32	—	6½	3000	Bunter sandstein.
5 Glucksbrunn, near Creuzburd	16	—	$1\frac{6}{10}$	250	Ditto.
V. *Hanover.*					
1 Luneburg	50	—	26	7200	Probably zechstein (*new magnesian limestone*), or bunter sandstein.
2 Rothenfelde	12	14	$7\frac{7}{10}$	1200	Gryphiten kalk (*oolite series*).
3 Hegersen	—	—	1½	120	Ditto.
4 Grossen Reiden	—	—	3	180	Ditto.
5 Munder	—	—	—	45	Ditto.
6 Salzhemmendorf	30	—	8	1400	Ditto.
7 Salzgitter	40	—	8	950	Bunter sandstein (*new red sandstone*).
8 Salzdettfurth	—	—	7	110	Ditto.
9 Salzderhelden	24	—	4	350	Ditto.

	Names of the Springs.	Depth of the springs in Germanfeet.	Temperatures. Degrees.	Contents, per cent.	Present produce in lasts of 4000 lbs.	Formations in which the Springs occur.
10	Sulbeck	16	—	$3\frac{1}{2}$	425	Ditto.
11	Bodenfelde	52	—	$1\frac{1}{2}$	300	Ditto.
12	Sülze	—	—	$3\frac{1}{6}$	100	Sand formation.
	VI. Brunswick.					
1	Julius Hall	70	—	11	14	Muschelkalk (*lias*).
2	Schoeningen	168	—	$5\frac{7}{10}$	260	Bunter sandstein (*new red sandstone*).
3	Salzdahlum	50	—	5	200	Ditto.
	VII. Electorate of Hesse.					
1	Carlshaven	70	8, 5	$1\frac{1}{2}$	300	Ditto.
2	Allendorf	104	12	5	4100	Ditto.
3	Schmalkalden	80	13	1	300	Ditto.
4	Nauheim	78	—	$2\frac{6}{10}$	3200	Ditto.
5	Rodenberg	95	10	3	200	Gryphiten kalk (*oolite series*).
	VIII. Landgraviate of Hesse.					
1	Homburg	—	—	—	100 ?	Bunter sandstein (*new red sandstone*).
	IX. Hesse.					
1	Soden	—	—	—	100	Ditto.
	X. Grand Duchy of Hesse.					
1	Salzhausen	36	—	[illegible]	118	Ditto.
2	Wissenheim	—	—	[illegible]	85	Ditto.
3	Budingen	—	—	—	75	Ditto.
	XI. Lippe Detmold.					
	Salz Uffeln	144	9	$6\frac{1}{2}$	500	Gryphiten kalk.
	XII. Principality of Waldeck.					
	Pyrmont	30	—	3	100	Muschelkalk (*lias*)?

Names of the Springs.	Depth of the springs in German feet.	Temperatures. Degrees.	Contents, per cent.	Present produce in lasts of 4000 lbs.	Formations in which the Springs occur.
XIII. *Kingdom of Wurtemberg.*					
1 Friedrichshall, near Jaxtfelde	524	12	27	3750	Aus steinsalz (*rock salt*) der zechstein formation (*new magnesian limestone*).
2 Offenau	400	—	28	—	Ditto.
3 Hall	332	—	27	2450	Ditto.
4 Sulz	350	13¾	24	200	Ditto.
5 Weissbach	830	—	22	75	Roth todt liegendes (*red dead lier, between the magnesian limestone* and *the great coal formation*).
XIV. *Grand Duchy of Baden.*					
1 Ludwigshall	420	—	28	3750	Aus stein salz und zechstein (*new magnesian limestone*).
2 Durrheim	425	—	27	—	Ditto.
3 Mosbach	380	—	1½	100	Roth todt liegende.
4 Bruchsal	—	—	—	100	Ditto.
XV. *Kingdom of Bavaria.*					
1 Reichenhall, with Rosenheim and Traunstein	46	8	1—23	15,500	Conglomerat kalkstein (nagelfluhe), which probably belongs to the zechstein formation.
2 Kissingen	78	—	—	500	Bunter sandstein (*new red sandstone*).
3 Orb	—	—	6	600	Ditto.
4 Phillipsthal	—	—	—	500	Ditto.
5 Durkheim	—	—	—	—	Probably roth todt liegende.
XVI, *The Swiss Alps.*					
1 Bex	819	8	1—25	400	Schiefer formation (uebergangs gebirge).
2 Moutiers	—	30	1$\frac{5}{16}$	425	Ditto.
XVII. *Alsace and Lorraine.*					
1 Dieuze	—	10	16	10,000	Gryphiten kalk.
2 Moyenvic	—	11	16		
3 Chateau Salins	46	14	14	2870	Ditto.
4 Vic	320	—	—	—	*Rock salt*, which may probably belong to the gryphiten kalk.
5 Lons le Saunier	—	—	6½	720	Gryphiten kalk.

The different formations yield the following proportions of salt:

1. Granite and slate formation:

Lasts of salt at	Bex	425
	Moutiers	400
		825

2. Porphyry and coal formation.

Lasts of salt at	Munster on the Stein	270
	Theodorshall	880
		1150

3. "Rothe sandstein formation" (red dead lier).

Lasts of salt at	Mossbach	100
	Weissbach	75
	Deirkbeck	—
		175

4. Zechstein, or alpenkalkstein formation (*new magnesian limestone*).

Lasts of salt at	Frankenhausen	2000
	Friedrichshall	3750
	Offenau	—
	Hall, in Wurtemberg	2450
	Sulz on the Necker	200
	Ludwigshall	3750
	Durrheim	—
	Reichenhall	15,500
	Luneberg	7200
		34,850

5. Bunter sandstein formation (new red sandstone).

Lasts of salt at	Durrenberg	6500
	Artern	1450
	Stassurth	1500
	Teuditz and Kotschau	480
	Koesen	1340
	Salzgitter	360
	Salzdettfurth	170
	Salzderhelden	350
	Sulbeck	425
	Bodenfelde	300
	Schöningen	280
	Salzdahlum	200

Lasts of salt at	Carlshaven	300
	Allendorf	4100
	Schmalkalden	300
	Nauheim	3200
	Salzhausen	112
	Weissenheim	85
	Budingen	75
	Homburg	100
	Soden	100?
	Kissingen	500
	Orb	600
	Phillipsthal	500
	Salzungen	3000
	Glucksbrunn	250
		26,567

5. Jungere kalk, between the bunter sandstein and the tertiary formations.

(*a*.) From the muschelkalk.

Lasts of salt at	Halle, in Saxony	6300
	Schonebeck	15000
	Sulze	800
	Lindenau	25
	Julius Hall	14
		22,139

b. Gryphiten kalk in Germany.

Lasts of salt at	Rolching	33
	Reine	330
	Rehme	700
	Rothenfelde	1200
	Hegersen	120
	Grossen Reiden	180
	Munder	45
	Salzhemmendorf	1400
	Salz Uffeln	500
		4508

c. Gryphiten kalk in Alsace.

Lasts of salt at	Dieuze and Mojensie	10,000
	Chateau Salins	2870
	Lons le Saunier	720
	Salins	3030
	Vic	—
		16,620

d. Green sand and chalk.

Lasts of salt at Königsborn near Unna.	3027
Werl	900
Sassendorf, near Soest.	640
Westerkatten	500
Salzkatten.	600
	5667
	48,934

6. Tertiary formations.

a. Sand formation (*London clay*).

Lasts of salt at Colberg	1500
Greifswalde	300
Oldesloe	1200
Sulz, in Mécklenburg.	440
Sulz, in Hanover	100
	3540

b. Mergel sandstein (gres a lignites according to Humboldt), from which arise the salt springs in Hungary, Gallicia, and the territory of Siebenburg, which yield, in lasts of salt, above 225,000

Lasts of salt from the granite and slate formation	828
Lasts from the porphyry and coal formation	1160
Lasts from the great older sandstone formation;—viz. the red sandstone (roth todt liegendes), and that which lies between the bunter sandstein and the zechstein.	61,592
Lasts from the newer limestones, viz. the muschelkalk (*lias*), the gryphiten kalk and Jura kalk (*oolite series*), and the chalk	48,874
Lasts from the tertiary formations, viz. the plastic clay (*braunkohlen format.*) and London clay (*sand format.*)	228,540

Article VIII.

An Examination of some Egyptian Colours. By James Smithson, Esq. FRS.

(To the Editor of the *Annals of Philosophy.*)

Sir, *Jan.* 2, 1824.

More than commonly incurious must he be who would not find delight in stemming the stream of ages; returning to times long past, and beholding the then state of things and men.

In the arts of an ancient people much may be seen concerning them: the progress they had made in knowledge of various kinds;

their habits; their ideas on many subjects. And products of skill may likewise occur, either wholly unknown to us, or superior to those which now supply them.

I received from Mr. Curtin, who travelled in Egypt with Mr. Belzoni, a small fragment of the tomb of King Psammis. It was sculptured in basso relievo which were painted.

The colours were white, red, black, and blue.

I have heard the white of Egyptian paintings extolled for its brilliancy and preservation. I found the present to be neither lead nor gypsum; but carbonate of lime. Chlorides of barium caused no turbidness in its solution. An entire sarcophagus of arragonite proves that the ancient Egyptians were in possession of an abundant store of this matter, remarkable often for its perfect whiteness. Was it the material of their white paint?

The red was oxide of iron. By heating, it became black, and returned on cooling to its original hue. In a case where so much foreign admixture was present, since the layer of red was much too thin to allow of its being isolated, I considered this as a better proof of red oxide of iron than obtaining prussian blue.

The black was pounded wood charcoal. After the carbonate of lime with which it was mixed had been removed by an acid, the texture of the larger particles was perfectly discernible with a strong lens; and in the fire it burned entirely away.

The blue is what most deserves attention. It was a smalt, or glass powder, so like our own, though a little paler, as to be mistaken for it by judges to whom I showed it; but its tinging matter was not cobalt, but copper. Melted with borax and tin, the red oxide of copper immediately appeared.

Many years ago I examined the blue glass with which was painted a small figure of Isis, brought to me from Egypt by a relation of mine, and found its colouring matter to be copper.

I am informed that a fine blue glass cannot at present be obtained by means of copper. What its advantages would be above that from cobalt, it is for artists to decide.

Intent upon the blue smalt, it unfortunately did not occur to me to examine, till I had washed nearly the whole of it away to waste, what was the glutinous matter which had been so true to its office for no less a period than 3,500 years; for the colours were as firm on the stone as they can ever have been.

A small quantity of it recovered from the water did not seem to form a jelly on concentrating its solution; or to produce a precipitate with galls. I imagined its vegetable nature ascertained by its ashes restoring the colour of reddened turnsol paper, till I found those of glue do the same.

The employment of powder of charcoal for a black would seem to imply an unacquaintance with lamp-black, and, perhaps, with bone black, and that of copper to colour glass blue, a deficiency of cobalt. And if the glutinous matter should prove, on a future examination, to be vegetable, our glue being then possessed may, perhaps, be deemed questionable.

Article IX.

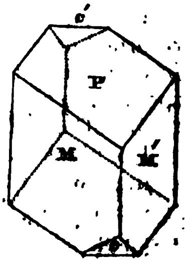
On the Crystalline Forms of Artificial Salts. By H. J. Brooke, Esq. FRS.

(*Continued from* p. 22.)

Sulphate of Nickel and Copper.

The *primary form* of this salt is an *oblique rhombic prism*, with an imperfect cleavage parallel to its lateral planes.

P on M, or M′	100° 15′
P on c′	117 30
M on M′	83 30



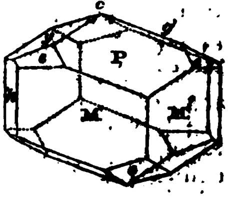
I received these crystals from Mr. R. Phillips, and having dissolved some of them in distilled water for the purpose of obtaining others with more perfect planes, I found that the first crystals deposited from the solution were *sulphate of copper*; the next, sulphate of nickel and copper similar to those which had been dissolved: these were removed, and were succeeded by a crop of the same crystals intermingled with a few others of the *rhombic sulphate of nickel*; and crystals of both these salts continued to be deposited together until the fluid was nearly all evaporated.

There were not any crystals of sulphate of copper deposited from this solution after the double salt began to crystallize, but the solution at Mr. P.'s, from which the crystals I received had been obtained, had subsequently produced crystals of sulphate of copper. It would appear from these circumstances that the double salt would only be produced in uncombined portions of both of the single ones.

Sulphate of Ammonia and Magnesia.

The primary form of this salt is an *oblique rhombic prism*.

P on M, or M′	104° 45′
P on e, or e′	154 40
P on g	135 40
P on c′	115 30
M on M′	109 30
M on k	125 15



The crystals of this and the following I have received from Mr. R. Phillips.

Sulphate of Copper and Potash.

The primary form is an *oblique rhombic prism*, and differing so little from the preceding in measurement, that the same figure may be used for both. The crystals do not appear to possess any distinct cleavage.

P on M, or M′	104°	30′
P on *e*, or *e*′	154	20
P on *c*′	116	20
M on M′	107	35
M on *k*	126	12

ARTICLE X.

On the Occurrence of Cleavelandite in the older Rocks generally. By W. Phillips, FLS. &c.

The discovery of cleavelandite as an ingredient in several rocks of various and distant parts of England and Scotland, as detailed in the *Annals* of November last, induced me to pursue the subject further, from the notion that if this mineral should be found only in particular descriptions of rocks, its presence, or the contrary, might tend to throw some light on the difficult and intricate inquiries connected with the comparatively relative ages of the older rocks in general.

With this view I have examined rocks of different countries, and although the examination has been limited to two or three hours each evening for three or four weeks, it appears to me that sufficient evidence has been obtained to evince the probability that the presence of this mineral is almost universal in the older rocks, and that it will frequently be found accompanying felspar in them.

Of the rocks of Cornwall, the only one noticed in my former communication was a porphyritic granite from Carnbrae; I have since found it intermingled with felspar, quartz, and mica, as the paste of a porphyritic granite from Huel Gorland Mine, near St. Die, and also in another from near the Land's End; in both the cleavelandite is white and opaque, and the felspar translucent or transparent. In a granite from Dartmoor, in Devonshire, consisting chiefly of nearly opaque red felspar intermingled with hornblende and quartz, the cleavelandite is translucent and slightly reddish, and it bears but a small proportion to the felspar. It occurs sparingly intermingled with felspar in the sienite of the Malvern Hills. I have not succeeded in finding this substance as an ingredient of any one of the numerous specimens in my possession from the porphyry or elvan dykes of Cornwall; the imbedded crystals being all felspar.

This mineral appears to be the only substance connected with hypersthene, in the hypersthene rock of Sky, for I have not been able to detect any felspar in the several specimens of that rock presented to me by Dr. Mac Culloch. I have detected it in contact with felspar in some fragments of granites from Tiree, presented to me for the purpose of examination by Capt. Vetch, as were also others from the Shetland Isles, Fula and Faira, in which also the cleavelandite occurs. To the same gentleman I am likewise indebted for a mass consisting of nearly white lamellar felspar and green cleavelandite in about equal proportions, simply adhering, not intermixed; as well as for an isolated fragment about an inch and a half square, of the latter mineral of a dark colour, having greatly the aspect of felspar, its cleavage planes being more than usually bright. Both these specimens are from Tiree.

The presentation to me of a box of rock specimens from Mont Blanc and its neighbourhood, by Charles Hampden Turner, Esq. with a catalogue "fourni par Joseph Marie Deschamps, à Servoz, Canton de Chamouni," afforded a favourable opportunity for their examination, especially as a large part of them, indeed all in which I found the cleavelandite, are noted in the catalogue as the "protogine granites of Jurine." In the specimen of the very summit rock of Mont Blanc, cleavelandite forms a considerable proportion, in connexion with steatite, talc, quartz, chlorite, and felspar; the cleavelandite being white, nearly opaque, and often considerably granular: it occurs even still more largely in the beautiful porphyritic granite of Pormenaz about two miles W or NW of Servoz, and in which the imbedded substance is a felspar of a delicate rose colour; the paste consists of cleavelandite, quartz, felspar, hornblende, and chlorite, and the aggregate is of a green colour. The rock of Vosa consists of dark lamellar felspar, imbedded chiefly in yellow and white cleavelandite including yellow mica. That of La Filla consists of nearly white and opaque cleavelandite, mica, chlorite, quartz, and felspar, and that of Brevent of the same substances. The rock of Des Trapettes consists chiefly of white cleavelandite and white mica (it is elastic), containing spots of yellow mica; neither quartz nor chlorite is perceptible in it; and much of the cleavelandite has a granular aspect when viewed in a direction contrary to that of the cleavage planes; two surfaces of this specimen are, however, coated by chlorite.

The only specimen of a rock in my possession from North America, is one consisting chiefly of black mica, with which transparent cleavelandite is intermixed; garnets are imbedded in it: this rock is from the banks of the Schuylkill, five miles W of Philadelphia.

In all the instances above enumerated, and in many more in which I have failed to discover the cleavelandite, it will be understood that the means relied on for its detection were simply those of cleaving from the rock minute fragments, and submitting them to the reflective goniometer—a test it may be assumed, sufficiently satisfactory, to render it needless to have recourse to the labours of the chemist, where the planes produced by cleavage are sufficiently bright, especially since the angles at which the planes of the cleavelandite meet each other are perfectly well ascertained, and essentially differ from those of felspar, with which this mineral was always confounded, until the error was lately detected by the nearly simultaneous labours of Lévy and Rose, and thereby furnishing evidence (if indeed any were wanting) to the real value of the mineralogical niceties belonging to cleavage and measurement, and their essential importance to every one who would become acquainted with the older rocks, which it may be said it is now the fashion to neglect for the more inviting and less laborious investigations requisite for the easier comprehension of the newer.

It is scarcely needful to say, that whenever felspar is mentioned as an ingredient of the before mentioned rocks, its presence was always ascertained by the goniometer.

In several of the rocks of Mont Blanc and the neighbouring mountains, it is however extremely difficult, if not impossible, to decide whether one of their ingredients be felspar or cleavelandite, without the assistance of the chemist, since the substance in question is often either considerably granular, or approaches the compact: sometimes, however, when that is the case, it becomes manifest by a studious search that this appearance only belongs to the cross fracture of the mineral, and that it does possess cleavages sufficiently distinct for the use of the goniometer; and in all cases where this has happened to me, the mineral has proved to be cleavelandite, not felspar: in the specimen from the Aiguille du Tour, which consists of a white granular substance having some appearance of cleavage, and which is rendered somewhat schistose by irregular layers of green chlorite, it is impossible to decide on the nature of the white substance, because the indications of cleavage are not sufficiently decisive for the goniometer, but the examination of many rocks from the neighbourhood leads to the conclusion that this substance is in reality cleavelandite.

If we regard the elements of the several substances constituting the rocks in which both felspar and cleavelandite occur, as having been in a state either of aqueous solution or of igneous fusion; that is to say, in a state in which the two alkalies entering separately into the composition of these two minerals, were at liberty to exert their affinities for other bodies, it seems very remarkable that they should each constitute separate minerals in connexion with very nearly equal proportions of the same earths—and as it occurred to me as being within the verge of probability that these two alkalies might, by coalescing and uniting themselves to the same or other earths, form another compound body which would probably in that case have for its primary form, one in which the planes would meet at angles different to those either of felspar or cleavelandite, I have been anxious to detect any such difference if it exists, but without success; for in every instance the angle obtained belonged to the primary form either of the one or the other of the above mentioned mineral.

It may be added that although these minerals are sometimes sufficiently distinct in the same specimen, there is no one character belonging to either which adequately distinguishes it from the other at first sight; the most important feature is, that in general the cleavage planes of felspar are more brilliant than those of cleavelandite; and hence it will be difficult hereafter to describe a rock in which one of these minerals appears to be an ingredient, without first having recourse either to the chemist or the goniometer, in order to prove that it is not the other.

Article XI.

Astronomical Observations, 1823.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Dec. 30.	Emersion of Jupiter's second satellite	11h	33′	34″	Mean Time at Bushey.
		11	34	55	Mean Time at Greenwich.
Jan. 8.	Emersion of Jupiter's first satellite	9	12	47	Mean Time at Bushey.
		9	14	08	Mean Time at Greenwich.
Jan. 11.	Emersion of Jupiter's first satellite	16	38	37	Mean Time at Bushey.
		16	39	58	Mean Time at Greenwich.
Jan. 12.	Occultation of a small star by the moon. Immersion	23	52	56	Siderial Time.
Jan. 12.	Emersion of Jupiter's third satellite	8	24	20	Mean Time at Bushey.
		8	25	41	Mean Time at Greenwich.
Jan. 13.	Emersion of Jupiter's first satellite	11	08	14	Mean Time at Bushey.
		11	09	34	Mean Time at Greenwich.

N. B. Jupiter's Limb was tremulous and ill defined.

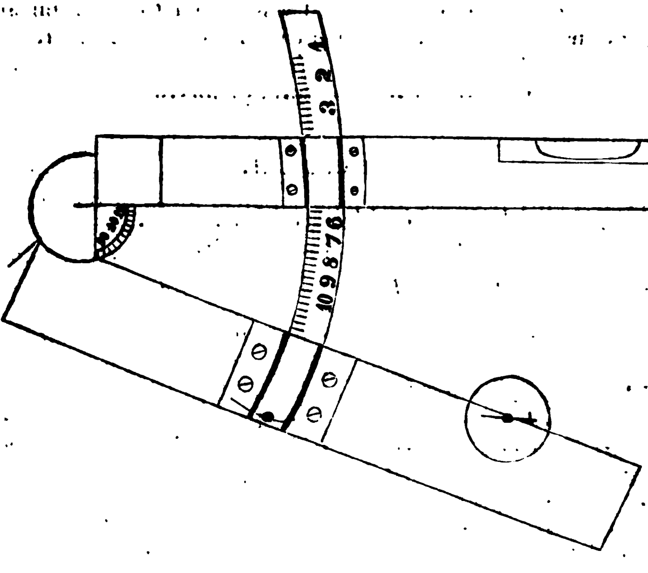
ARTICLE XII.

On an Improvement of the Clinometer. By M. P. Moyle, Esq.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Helston, Jan.* 1, 1824.

I HAVE made an addition to the clinometer now generally in use, which I flatter myself is an improvement; at least its utility is more particularly felt in this neighbourhood by the superintendants of mines in calculating the dip or underlie of a lode. Its former construction pointed out only the angle from the horizon, of any stratum, &c. I have now added another quadrant, on which is delineated the number of feet and inches a lode will underlie at certain angles in a fathom, perpendicular depth. The following is an outline of the instrument.



I find the underlie of a lode of one foot in a fathom will be indicated by an angle of 80½° from the horizon, six feet in a fathom at the angle of 45°, and so on.

I am, Sir, your humble servant,

M. P. MOYLE.

ARTICLE XIII.

Reply to the Review of Mr. Gray's Elements of Pharmacy. By Mr. S. F. Gray.

(To the Editor of the *Annals of Philosophy.*)

SIR, *18, Burton-street, Burton Crescent, Dec. 23, 1823.*

I BEG leave to thank you for the commendation you have given to my Elements of Pharmacy, in saying that it is calculated to convey a considerable portion of information; but I must at the same time desire your attention to a few points in the sequel of your review.

The arrangement is partly taken from Stahl, partly from Biot, and is such as best suited my purpose of training the student to scepticism, or, at least, indifference in theoretical points.

The definition to which you object is that of Black, with the addition of the last clause, which I was induced to add, because chemists, from the analogy of electricity to galvanism, attempt to explain the phenomena of the former power, although it is not produced by alteration of temperature, or mixture of bodies, but by mere mechanical means, and therefore will not come within the limits of the original definition of Dr. Black. Heat and cold are used in the definition in their popular sense, as two contrary powers, not only because the philosophical ideas of temperature and caloric had not yet been mentioned; but also because the existence of a frigorific principle having been started, and its partisans not yet extinct, the popular expression, which involves no theory, best suited the cautious character of a sceptic.

The first error which you say you shall notice occurs in p. 95, and after quoting the passage, you say, "it proves incontestably that Mr. Gray is ignorant of the composition of *sulphuric acid*, for he has once in words, and three times by symbols, misstated its nature." But I believe you will upon reflection agree with my statement, that *sulphuric acid* consists of a charge of sulphur united with three of oxygen. I acknowledge that I have certainly erred in respect to oil of vitriol; but having written *ten* for *one* in the first instance, the subsequent repetitions were mere slips of the pen. How easily these occur your own use of the words sulphuric acid for oil of vitriol in this passage shows, and we shall see more proofs hereafter. As it was only intended in this place to explain the mode of using the symbols, the slip is here of no consequence, and indeed it turns out better adapted for that purpose than the truth would have done, as introducing an example of a coefficient sign. In the history of oil of vitriol, p. 138, the composition is stated right.

In commenting on my exposition of Berzelius's laws of combination, you object to my mentioning the speculations of that chemist on the existence of oxygen in ammonia, in an elementary treatise. This is mere matter of opinion. Mr. Brande indeed does not make any mention of the supposed compound nature of nitrogen; but both Mr. Henry (8th edit. vol. i. p. 243) and Dr. Thomas Thomson (6th edit. vol. i. p. 214) not only notice the suspicion of azotic gas, and ammoniacal gas containing oxygen, but the one notices the ammonium of Sir H. Davy, and the other the nitricum of Berzelius. So that I have just cause to be astonished at your saying, "If the student after reading this passage were to look into the chemical works of Thomson, Henry, or Brande, he would find no mention either of oxygen or nitricum existing in ammonia."

My idea of the composition of the liquor plumbi acetatis agrees with the statements of Dr. Thomas Thomson, Mr. Henry, Mr. Brande, and Mr. Anthony Todd Thomson. I am again astonished at your saying, that "in p. 81 no rules are given for *describing* those salts that contain an excess of base." I know not how to account for this, unless by supposing a whole paragraph has dropped out of your copy at press, as single letters sometimes do; for the third paragraph in that page, consisting of no less than 14 lines, expressly relates to the mode of naming (not indeed *describing*, as you have by another slip worded it) salts when an acid combines in different proportions with the same base, and five modes of nomenclature are related; and in the last paragraph of p. 94, the subject is resumed.

You then proceed to say (for I must now quote your words at length), "On the same ground we object to the following statement: Moist iodine added to phosphorus yields a sour colourless gas, which is rapidly absorbed by water, and must be collected in a quicksilver apparatus; a gallon of this gas weighs about 311 grains. Here the changes are either $I^5 + P$ into $I^5 + P^1$; or $I + P + H^1$ into $I H + P^1$, and the new acid is called the iodic or hydroiodic." "The pupil would naturally suppose that Mr. Gray considers the iodic or hydroiodic acids (properly hydriodic) as similar; but he ought to have known that iodic acid consists of oxygen and iodine, and the hydriodic acid of hydrogen and iodine; it is the latter only which is formed, excepting a quantity of phosphorous acid, of which no notice is taken, nor is the decomposition of the water even hinted at, although the formation of the hydriodic acid depends upon it. (P. 166.)"

These are your precise words. I pass over any inquiry as to the ground upon which you object to what I have said; although I am not able to discover any connexion between this passage and that immediately preceding it, respecting the subsalts; but proceed to your assertion, that I have taken no notice of the phosphorous acid which is formed. This shows how easily slips of the pen may be made, and may abate your astonishment at a slip of my own; for here after you had twice copied from my book the symbol for phosphorous acid, P^1, as being a product of this experiment, you assert, within the compass of only six lines, that I have taken no notice of its production. Nor is this all, for you go on to say I have not even hinted at the decomposition of the water. Is it possible that the Editor of the *Annals of Philosophy* can be unable to read a chemical theorem expressed in symbols? Upon no other ground can this censure, and the one hitherto passed of my not knowing the difference between iodic and hydroiodic (for so I shall continue to spell it) acids, be explained. The two formulæ I have given in symbols express two theories of this experiment. The first I^3, P changing into I^2, P^1 expressing that of Berzelius, in which iodine, I^3, is considered as a superoxide of iodinum containing three charges of oxygen, which, being acted upon by phosphorus, P, a simple body transfers one charge of its oxygen to the phosphorus, forming phosphorous acid, P^1, while the iodine having of course only two charges of oxygen left united with it, is changed into the iodic acid, I^2, of Berzelius, not of Gay-Lussac: in this theory, the water is passive. The other formula I, P, H^1, changing into I H, P^1 expresses the theory of Gay-Lussac. Here iodine, I, and phosphorus, P, are both considered as simple bodies, and the water, H^1, is active in the experiment, and is indeed decomposed, its hydrogen, H, uniting with the iodine and forming hydroiodic acid, I H, while its oxygen 1 being thus set free unites with the phosphorus and forms phosphorous acid, P^1. By this developement of the symbolic formulæ, how little cause there was for your censure is evident. As these experiments on iodine are of no use in pharmacy, I should not have mentioned them at all, leaving them to the pure chemists to detail; but that there existed a wide difference between the views which Berzelius and Gay-Lussac have taken of the subject. It was this slight connexion between them and the professed object of my work, which caused me to express these incidental matters in symbols, partly as an exercise to the student, and partly because Berzelius's opinions on iodine, as detailed in the *Annals of Philosophy*, have not been noticed in any of our elementary treatises on chemistry, and I was anxious that they might be brought forward.

As to the use of the term hydroiodic acid, instead of hydriodic, it may surely be justified by this single observation. The union of hydrogen with chlorine produces, according to Gay-Lussac's nomenclature, the hydrochloric acid; with sulphur, the hydrosulphuric acid;—with tellurium, the hydrotelluric acid;—with cyanogen, the hydrocyanic acid;—with phthore, the hydrophthoric acid; so strict analogy requires that the union of hydrogen and iodine should produce the hydroiodic acid, and our own language is not so rich in vowels that we can afford to reject those which are offered to our enjoyment. There is indeed little doubt but that the term hydriodic was an accidental error of Gay-Lussac, or of his printer, and has been perpetuated like the etiolation of Fourcroy in his Système, instead of etoilation; or the laplysia of Linnæus in his Systema Naturæ, 12th edition, instead of aplysia; which have been so often repeated, that they are become standard errors, and bid fair to supplant the true orthography, and disconcert the etymologists.

Your observations respecting arsenious acid are partly just; the directions respecting testing for arsenic are, I confess, imperfect and incorrect; but it was not my intention to enter upon the subject of forensic medicine; and I have always taught my own pupils that nothing short of the production of true metallic arsenic will justify a medical practitioner to condemn a supposed poisoner in cases which are in any way dubious. Fortunately as arsenic is cheap, and almost always used by ignorant persons, they administer it in such quantities that dubious cases seldom occur. I cannot, however, possibly conceive how you could make the following assertion:—"Nor is the direct evidence by metallization in any way alluded to," unless by again supposing that another whole paragraph has, by some strange fatality, dropped out at press from your copy; for the very next paragraph, in p. 151, to the one you quote, treats of the metallization of white arsenic by the addition of a subcarbonate of potasse and charcoal dust.

Again passing over for a moment one of your observations, I proceed to the concluding paragraph, which is indeed the cause of my troubling you with this notice of your review. Man is always subject to error, and therefore the mention of a few errors in my Elements, which I am conscious must, like every work hitherto published, or that ever will be published, contain a few, and I hope but a few, would not have been any other way noticed by me, but by silently amending them in any future work. But I am charged with dividing chemists into rational and irrational, and placing Sir Humphry Davy among the irrational chemists. Now I think I may fairly challenge you to produce the word *irrational* from any part of the book. A cautious habit of distinction, that I have acquired, and of the want of which in the generality of publishing chemists I have several times complained, has led me to distinguish between pure chemists, pharmaceutical chemists, and I believe technical chemists, not as words of reproach, but of distinction. So I have mentioned publishing chemists to distinguish them from mere amateurs, or those who either labour only for themselves, or those who are too modest to appear before the tribunal of the public. And again in point of theory, every professor, and indeed almost every chemist, has some peculiar doctrines of his own; but they may, and accordingly have been by me, divided into two classes, according to the greater or less use they make of analogical reasoning in their theories. Formerly from the want of a sufficient number of facts, analogical reasoning was the sole basis of chemical theory, as may be seen in Becher; by degrees direct evidence has been obtained on certain points, but not yet on all. Berzelius and some others, among which, did not my habitual indifferentism hinder me, I should rank, are of opinion, that the analogical reasoning of our forefathers should be retained so long as the facts which come to light can possibly be explained by their hypotheses, and even in case this can no longer be done, we should make as little change as possible. These are the chemists I have denominated rationals, not from any superiority in understanding which they may claim, but from their greater use of analogical reasoning than is allowed by the opposite class; analogical and rational being constantly used in science as synonymous; and the latter word being best adapted to be used as a substantive. The peculiar dogma of the other school is to reject all analogical reasoning, and consider every body which has not yet been separated into others as simple. To these chemists, which include the generality of modern professors and authors, I have given not the name of irrationals as you state, and which I expressly avoided lest I should unintentionally offend, but that of Epicureans; because Rouelle, who may be considered as the father of the sect, is said to have inscribed on the walls of his lecture room, a saying ascribed to Epicurus; That we can know only what we perceive by our senses.

No man has a higher respect for Sir Humphry Davy, as a chemist, than myself. Of his two precursors in the same path, Homberg and Scheele, it is only the latter that can enter into any competition with him; and if the Pomeranian exceeds in the number of new substances he has discovered, the Englishman has by far the advantage in the greater practical advantages to be derived from his labours.

As to my opinion, however, of Lavoisier, which I have deferred to this place, it is unaltered by your remarks. He indeed turned chemistry upside down, and he has shown us that the phenomena of chemistry may be explained by thus reversing its former theory, but theories are such tottering things that I do not consider this as any great feat. I still deny that chemistry is deeply indebted to him, in any other way than that the total change he proposed, coinciding with the political circumstances of the times, was brought into more notice than it would otherwise have obtained, and thus it became necessary for the cultivators of chemistry either to defend the old doctrines, or advocate the new; and in this manner indeed he has indirectly been of the greatest service to it. How often are his experiments referred to? Seldom;—I might say, never. Are they considered as examples of the most scrupulous accuracy like those of Cavendish? Far from it. That you may not suppose this is only my own prejudiced opinion, I refer you to the opinion expressed by Dr. Thomas Thomson in his catalogue of Lavoisier's writings, in the second volume of the *Annals of Philosophy*. I remain, Sir, yours respectfully,

SAMUEL F. GRAY.

ARTICLE XIV.

Remarks upon the preceding Answer. By R. Phillips, FRS. &c.

(To Mr. Gray.)

SIR,

IN the observations which I shall offer on your reply, I shall confine myself as much as possible to matters of fact. I must, however, confess, that I was completely in error respecting your motive for adopting the arrangement which I criticised. Had I known that you were assembling gum arabic, borax, hemlock leaves and eggs, as "farinaceous bodies," for the purpose of exhibiting an absurdity, I should have congratulated you on the success of your exertions; but I think you should have rendered your motive evident; for the reader may imagine that you are so unhappily circumstanced as really to suppose that eggs are farinaceous, and oyster shells combustible.

I know not from whence you have copied the definition of chemistry which you attribute to Dr. Black; there are two definitions given in his lectures, and both so unlike that which you ascribe to him, that neither contains the word "cold."

You admit that you have four times misstated the composition of sulphuric acid, by assigning it ten atoms of water instead of one atom; but you say, that in p. 138 it is stated right. On referring, I find that it "is supposed to consist of S^3 + water." Now, according to the theory of "*slips*," of which you have made so much use, I would ask, whether the pupil having four times learned that oil of vitriol contains 10 atoms of water, he would not be likely to conclude that at p. 138 the coefficient had slipped out, rather than that it had three times previously slipped in? According, however, to Berzelius, S^3 + water, would express a compound of three atoms of sulphur + one atom of water; so that I deny you having rightly stated the composition of sulphuric acid.

I again assert that neither Thomson, Henry, nor Brande, mentions the existence of oxygen or nitricum in ammonia. It is not necessary to quote at length; Dr. Thomson (System, vol. ii. p. [illegible]) says, it is "a compound of three atoms of hydrogen and one atom of azote." Dr. Henry (Elements, vol. i. p. [illegible]) also represents it as "constituted of three atoms of hydrogen = 3 + 1 atom of nitrogen = 14;" and Mr. Brande, of "13 of nitrogen and 3 of hydrogen." (Manual, vol. i. p. 364.) That Goulard's extract of lead is a sub-binacetate and not a subtritacetate is an assertion which I again repeat. You will observe that three authors out of four whom you quote in support of your opinion, infer the composition of this acetate of lead, not from analysis, but from an experiment of Berzelius; the fourth authority, Mr. A. T. Thomson, is so far from asserting this compound to be a subtritacetate, that he quotes the very analysis upon which I founded my conclusion of its nature. Mr. Thomson says (London Dispensatory, p. 693), "According to the experiments of Dr. Bostock, the constituents of 100 parts of the saturated solution are 23·1 of oxide, 5 acetic acid, and 71·9 of water, which," he adds, "agree with the statement of Thenard, who found that the salt, when crystallized, consists of 17 parts of acid, 78 of oxide of lead, and 5 of water, in 100 parts." Now, it will appear, that the quantities of acid and oxide by Dr. B.'s analysis, are to each other in the proportion of 50 acid and 231 oxide; and in Thenard's analysis as 50 to 230, very nearly. According to Dr. Thomson, Dr. Henry, and Mr. Brande, acetate of lead is composed of 50 acid + 112 oxide; and you will, perhaps, admit that, supposing Goulard's extract to be a sub-binacetate, it must consist of 50 acid + 224 oxide; but if a subtritacetate of 50 acid + 336 oxide; the question, therefore, depends upon whether 231 or 336 are nearer to 224, and I think I may safely leave it to you to decide.

I must again insist that at p. 81 of your Elements, no rules are given for describing those salts that contain an excess of base. The 14 lines to which you allude have not *slipped* out of my copy; but if these 14 lines ever contained a syllable respecting sub-bisalts or sub-trisalts, such syllable has indeed disappeared. In p. 94, to which you refer me for a continuation of the subject, all that is contained respecting it is, that to express the number of charges, the terms employed are, "subsesquiphosphate, sub-bisulphate, for salts with excess of base." Not a word occurs respecting subtrisalts, those which occasioned the remark that I made. I admit that in p. 151 of the Elements of Pharmacy, there is a paragraph which treats of the metallization of white arsenic by the addition of "a subcarbonate of potasse and charcoal dust;" but I deny that it has any connexion whatever with the subject of detecting arsenic. It is stated without reference to it, and just as you would mention the reduction of any other oxide: indeed totally imperfect as the method which you had previously stated is, it is given without any reservation, and as possessing such a degree of certainty as would render all additional proceedings useless.

When I asserted that you had not noticed the decomposition of water, and the formation of phosphorous acid, which occur during the action of moist iodine upon phosphorus, I alluded merely to your verbal statements; and I will readily confess to you that I did not understand a chemical theorem expressed in such symbols as you employ.

In the first place, in giving your symbolic statement of Berzelius's theory, you represent his superoxidum iodicum by I^3, but he by $\dddot{I}$; your symbol means 3 atoms of iodicum, not iodicum combined with 3 atoms of oxygen; $\dddot{P}$ is Berzelius's representative of phosphorous acid, consisting of phosphorus + 3 atoms of oxygen; your symbol of P^1, if it have any meaning, is that of 1 atom of phosphorus. Again, you represent the "iodic acid of Berzelius" by I^2, which, according to him, means 2 atoms of iodicum; his symbol is $\ddot{I}$.

I observe also, that you suppose the phosphorous acid of Berzelius to contain only 1 atom of oxygen, whereas he states it to contain 3 atoms. On this subject I may remark, that you have performed a feat on paper which Berzelius himself would be glad to see reduced to experiment; if you find that his phosphorous acid is formed without the decomposition of the water, you are of course able to present him with his imaginary iodicum in an isolated state; for it must have surrendered its 3 atoms of oxygen to the phosphorus.

The hydriodic acid of Gay-Lussac, which you allow to be formed in this experiment, is *not* the iodic acid of Berzelius; he terms it iodas hydricus,* and represents it by Aq $\ddot{I}$, and not by $\ddot{I}$, as he would do if it were mere iodic acid, and still less by I^2, as you have done, which, as I have already observed, means 2 atoms of iodicum.

In explaining Gay-Lussac's theory, you have employed H^1 to express an atom of water; whereas in your "right" statement of the composition of sulphuric acid, you employ the word at full length, thus; "S^3 + water;" Berzelius, however, adopts Aq, and if H^1 have any meaning at all, it designates one atom of hydrogen. In order to represent the oxygen of the atom of water, you separate the coefficient thus, $\underline{1}$; while Berzelius represents it by O, and 2 atoms of oxygen by O^2, but $\underline{1}^2$ would be rather an odd expression for the same meaning.

I have now done with your symbols, and nearly with my remarks. If the errors you have committed have produced any effect upon me, it is that of confirming my utter dislike of symbolic notation. When explaining the motives that induced you to adopt an arrangement which to me appeared of a most extraordinary description, you have attributed to it the advantage of training the student to scepticism or indifference in theoretical points;

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* Essai sur la Théorie des Proportions Chimiques, Table, p. 118.

and you almost congratulate yourself upon the errors committed with respect to sulphuric acid, as better adapted for your purpose than the truth would have been; what happy advantage you may discover in the utter confusion in which you have involved the Berzelian symbols, I cannot imagine; and although your student may, in the commencement of his career, acquire scepticism or indifference in theoretical points, he must, I think, terminate it in the belief that you do not possess "that cautious habit of distinction" which it is your boast to have attained.

Yours, &c. R. Phillips.

Article XV.

On the Detection of small Quantities of Arsenic. By T. S. Traill, MD. &c.

(To the Editor of the *Annals of Philosophy.*)

Dear Sir, *Liverpool Royal Institution, Jan. 16, 1824.*

Your very interesting paper in the last number of the *Annals*, on the tests of arsenic, followed by an application to examine the contents of a stomach in which I discovered that deleterious substance, has drawn my thoughts to a subject which has often engaged my attention, from having been repeatedly called on to determine the nature of substances found in the alimentary canal of persons who have died under suspicious circumstances.

Your remarks on the application of the usual liquid tests are extremely just, and, in my opinion, leave nothing further to be desired on the subject; but I have long been aware of the erroneous opinion entertained by authors of reputation, respecting the inutility of attempting to reduce to the metallic state, portions of white arsenic considerably less than a grain. Even by the common process, I have often succeeded in reducing to a perfect metallic film much less than half a grain of the arsenious acid; although it was the opinion of the celebrated Black, that one grain, and of Dr. Bostock that three-quarters of a grain, are the smallest quantities from which we can hope distinct results by this process. I may here notice the importance of attempting the reduction in all cases of suspected poison; for it has several times happened, that I have been able to show the presence of arsenic in cases where it has been supposed absent, because the white powder gave no alliaceous smell when thrown on a *live coal*. This method of operating should be abandoned; because the carbonic acid and sulphureous vapour from the coal disguise the peculiar odour of the arsenic; and much of the powder is probably raised in vapour without reduction, and consequently without giving out the alliaceous smell.

The trouble and delay of preparing a coated tube induced me long ago to attempt the reduction in a thin glass tube, with the naked fire, or with the blowpipe; and the facility thus obtained made the reduction much less irksome. But your happy suggestion of the spirit-lamp has rendered the process still more easy—even elegant; and has enabled me to produce unequivocal metallization of arsenic from portions of the white powder, far more minute than what has been mentioned—a circumstance of no small moment, as it affords the most unexceptionable test of what may affect the life of a human being.

The following simple apparatus is all that is requisite; and after many trials, I give the process about to be described the preference.

Take a thin glass tube 2¼ inches long, and about 0·4 inch wide, closed at one end, with a slightly dilated mouth; like the common test tubes of the blowpipe apparatus. A piece of copper wire, loosely twisted round its upper end, serves to attach it to any convenient support, at an angle of about 30° or 40°; while the flame of a spirit-lamp is applied to the closed end of the tube containing the mixture to be reduced.

I employ either the black flux, or a little subcarbonate of soda or potash mixed with charcoal powder. Either of these should be at least equal to thrice the weight of the substance to be ascertained; but a small excess of them is safer than the opposite extreme. Where the quantity is not very minute, it is unnecessary to grind the whole ingredients together, but mixing them on a piece of glass, or of writing paper, with the point of a knife, before they are introduced into the tube, will be sufficient. The tube should be dry and clean; and its orifice may be slightly stopped with paper. Where the quantities are very minute, I usually grind them in an agate mortar; but as every such manual operation, however simple, is embarrassing to those little habituated to experiment, I consider it very useful to simplify the whole process by omitting this operation, and substituting an addition of charcoal powder, after the mixture is introduced into the tube. The flame of a spirit-lamp will speedily raise the closed end of the tube to a dull red heat; and in less than two minutes, a shining metallic crust will invest the upper side of the inclined tube, about half an inch from the flame. When the tube is cold, I shake out the loose materials from the bottom, and then scrape off the metallic crust from its sides with a knife. This saves the tube for further use, a matter of some consequence in the country. By this process I have reduced less than one-eighth, and even than one-tenth of a grain of arsenious acid, or, as it is sometimes called, the white oxide, to the metallic state. The metallic crust afforded by one-tenth of a grain, was perfectly distinct to the naked eye, and very conspicuous when a lens was employed. The scrapings of the tube in this case glistened strongly, with a metallic lustre, on the clean blade of the knife; and when six different portions of this substance were projected on a poker, heated to a dull red, each gave a distinctly visible white smoke, and decided alliaceous odour, although each portion could only have contained about 1/98th of a grain of the metal.

I may add, that if a clean knife be held in the fumes, a portion of a white powder will be condensed on the blade, even from the most minute portions of arsenic which I have thus volatilized.

I am, dear Sir, very truly yours,
THOMAS STEWART TRAILL.

ARTICLE XVI.

On the Expansion of Gases. By Mr. Matthew Biggs.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR, *Jan. 15, 1824.*

A GENTLEMAN signing himself X. has, in the Philosophical Magazine for December, given some algebraic formulæ for calculating the expansion of gaseous fluids under increasing temperature, which, he says, prove the accuracy of certain rules for the same purpose, which I referred to in a paper you were good enough to insert for me in the *Annals of Philosophy* for December, and the inaccuracy of some which I proposed to substitute for them. It is so evident that he is wrong, that I had not intended to take any notice of what he says, but having been told that some answer ought to be made, I may just observe, that any person who will take the trouble of working the same sum by his method, at one operation and at several, may satisfy himself that no dependance whatever is to be placed upon it; thus, let 100 volumes of gas at 32° be raised to 212°,

$$\frac{100\,(480 + 180)}{480} = 137{\cdot}5,$$

This is correct according to Gay-Lussac and others.

Now let 100 volumes of gas at 32° be raised to 212° by four several additions of 45°,

$$\frac{100\,(480 + 45)}{480} = 109{\cdot}375,$$

$$\frac{109{\cdot}375\,(480 + 45)}{480} = 119{\cdot}6289,$$

$$\frac{119{\cdot}6289\,(480 + 45)}{480} = 130{\cdot}8441,$$

$$\frac{130{\cdot}8441\,(480 + 45)}{480} = 143{\cdot}1107,$$

upwards of 5½ volumes more than the truth; and if the elevation of temperature be supposed to take place by additions of single degrees, it is clear that the error must be immense.

The gentleman is pleased to observe at the close of his paper, that he has shown the failure of my attempt to correct the rules which have been given for making the above calculation, and that the one which I have proposed is not *perfectly* accurate. How he has proved this, I am at a loss to discover; but I imagine that whether my results are accurate or not, his will form no standard by which to judge of them.

I am, Sir, yours respectfully,
MATTHEW BIGGS.

ARTICLE XVII.

Account of a new Mineral Substance. By M. Levy, Esq. MA. in the University of Paris.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Jan. 20, 1824.*

AT the suggestion of Mr. Heuland, I propose to give the name of Bucklandite (in honour of the celebrated Professor of Oxford), to a mineral substance, the crystallographical characters of which I find to differ from any hitherto described.

I have observed this new mineral upon a specimen belonging to Mr. Turner's collection; it was placed with the pyroxenes, to which substance it has a great resemblance of form and external characters. It occurs in small crystals represented by figs. 2 and 3; their colour is brown, nearly black, and they are opaque. They easily scratch glass, and appear to me to be much harder than pyroxene. I could not find any cleavage parallel to the natural planes of the crystals, nor in any other direction. The

Fig. 1.

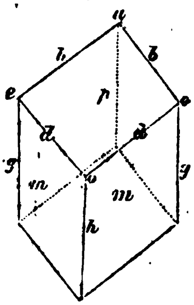


Fig. 2.

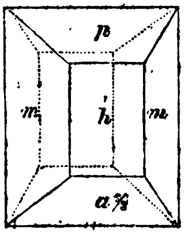
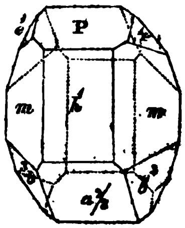


Fig. 3.



specific gravity and analysis I cannot give without spoiling the specimen, because a sufficient quantity of the substance could not be detached for the purpose of experiment.

From figs. 2 and 3, it is obvious that the forms of the crystals which they represent may be derived from an oblique rhombic prism, and that by assuming the faces I have marked m as the lateral planes of the primitive, and that marked P as the base, all the others will be the result of simple laws of decrements. An oblique rhombic prism may, therefore, be assumed as the primitive form of this substance; but to the same class of prisms belongs also the primitive form of pyroxene; it remains, therefore, to examine whether those two primitive forms are connected by any simple mode of derivation; and first, in order to give greater weight to the result of this investigation, it is necessary to mention that although most of the crystals have their faces covered with a greyish earthy substance, which render them unfit to be measured by the reflecting goniometer, it is, however, possible to measure some of the incidences by that means, by rubbing off previously the earthy matter which covers the planes.

If the crystal represented by fig. 2, is derived from the primitive of pyroxene, the faces marked p and h^1 must be either the results of decrements upon the angles a and o of the primitive, or the one being the result of such a decrement, the other must be the base of the primitive, or the face which replaces the edge h, and is the result of a decrement by one row upon that edge. Now let n be the law of decrements, which would produce upon the angle a of the primitive of pyroxene a face corresponding to one of the two faces p and h^1, fig. 2, and n^1 the law which would produce the other upon the angle o, let h be the lateral edge of the primitive of pyroxene 1, being the oblique diagonal of the base. Let α represent the angle of the base of the primitive of pyroxene with the edge h, and let β equal the angle of the two faces p and h^1, fig. 2; that is to say, equal to (a^n, o^{n1}). Then the following formula will easily be found:

$$\text{Tang.}\,(a^n, o^{n1}) = \text{tang.}\,\beta = \frac{h \sin.\,\alpha\,(n + n^1)}{n\,n^1 + (n - n^1)\cos.\,\alpha - 1}.$$

Now, if in this formula the numerical values of h, α, β, corresponding to the particular case we are considering, are substituted, it will be found that no simple values of n and n^1 can satisfy the equation, nor could any simple value be obtained for the one if the other was supposed to be equal to nothing, or infinity; that is to say, if one of the faces p or h^1, fig. 2, was supposed to be parallel to the edge h of the primitive of pyroxene, or to the base of the same form. It appears, therefore, that this substance does not belong to pyroxene; the only other mineral to which it bears some analogy is amphibole, but this last substance cleaves always very easily parallel to the lateral planes of the primitive, and besides it might be proved that their forms are incompatible in the same way as it has been done for pyroxene.

From the measurements I have taken, which very nearly agree with those Mr. W. Phillips was so kind as to take upon a small crystal I gave him of this substance, I am led to take for the primitive form, an oblique rhombic prism in which the two lateral planes are inclined to each other at an angle of 70° 40′, the base upon either of the lateral planes at an angle of 103° 56′, and in which the ratio of one side of the base to the lateral edge is nearly 100 to 97.

The other incidences are,

$$(m, h^1) = 125^\circ\ 20)\ (p, e^1) = 121^\circ\ 30'\ (p, b^3) = 95^\circ\ 40'$$

$$(p, h^1) = 114^\circ\ 55)\ (p, a^{\frac{2}{3}}) = 99^\circ\ 41'\ (m, b^3) = 160^\circ\ 24'$$

These crystals are accompanied by large green opaque crystals of scapolite, lamellary black hornblend, and flesh-coloured laminary carbonate of lime. The specimen comes from the mine Neskiel, near Arendal, in Norway.

Article XVIII.

Corrections in Right Ascension of 37 *Stars of the Greenwich Catalogue, together with an Inquiry how far it would be advisable that the Daily Corrections in R.A. and North Polar Distance of the* 46 *Zero Stars should be computed Annually at the Public Expence.* By James South, FRS.

(*Concluded from* p. 45.)

Having in the former part of this paper asserted, that the corrections in right ascension of the principal fixed stars, cannot be obtained piece-meal without considerable trouble, and occasional error, we will now see how far the procuring corrections in polar distance, is subject to similar inconveniences; in doing this, we will accompany the practical astronomer, first into the observatory, secondly into the computing room.

The instrument commonly employed for determining polar distances, is a telescope attached firmly to a graduated circle, and by means of micrometer microscopes placed opposite various parts of its divided limb, the errors of eccentricity, division, and partial expansion, are either destroyed, or rendered almost insensible. The observations are generally conducted in the meridian, and are extremely simple, consisting only in the accurate bisection of the star, by the horizontal wire, situate in the focus of the object glass; this done, the different microscopes are read off, and the mean gives the observed polar distance of the star.

To this, however, corrections must be applied; *generally* for refraction, and *always* for aberration, lunar nutation, solar nutation, and variation, ere the index error of the instrument can be found, by a single zero star; and in proportion as many, or few of these stars are observed, will the index error be well, or ill determined; and the importance of its being accurately ascertained cannot, perhaps, be rendered more evident than by stating, that index error is to observations with the circle, what the clock's error is, to observations with the transit instrument. But lest I should be suspected of exaggerating the difficulties of procuring, either the corrections in right ascension, or those of polar distance, I will here present the reader with a specimen of each.

Greenwich Observations for 1821, *Nov.* 27.

α Lyræ.

Observed right ascension	18^h 30′ 46·33″
Observed polar distance	51° 21′ 33·9″
Barometer	29·54 inches.
Thermometer	46°

Correction for Right Ascension.

Equation for aberration, precession, and solar inequality of precession.

By Maskelyne's Table 17, column α Lyræ, Nov. 26 = + 0·38″
Dec. 6 = + 0·30

Therefore diff. for 10 days ...... = − 0·08″
And diff. for 1 day .... = − 0·008

The equation, therefore, for Nov. 27 will be + 0·38″ − 0·008″ = + 0·372″.

Equation for Nutation.

By reference to the Nautical Almanac, and by the help of a little calculation, the place of the moon's node on Nov. 27, is 10^s 29^d 35′.

By Maskelyne's Table 18, column α Lyræ 10^s 20^d = + 0·41″
11 0 = + 0·30

Therefore diff. for 10 degrees .... = − 0·11″
And diff. for 25 minutes .... = − 0·005
Now − 0·11″ − 0·005″ = − 0·105″.

Equation, therefore, for 10^s 9^d 35′ = + 0·41″ − 0·105″, = + 0·305″; and + 0·372″ + 0·305″ = + 0·677″, = the correction required.

Mean RA of α Lyræ Jan. 1821 ...... = 18^h 30′ 52·92″

Correction for Nov. 27 .... = + 0·68

Apparent right ascension .... = 18 30 53·60

Observed right ascension .... = 18 30 46·33

Clock's error .... = − 7·27

Correction for North Polar Distance.

Equation for refraction.

Greenwich observ. 1812, p. 251, log. mean refraction. = 1·11261

1811, p. VI. + log. of bar. 29·54, and therm. 46° = ... 0·00347

Log. of true refraction = ... 1·11608

True refraction = 13·05″.

Equation for Aberration.

Sun's longitude by Nautical Almanac for Nov. 27 = 8^s 4° 58′

Gr. obs. 1813, p. 260, column α Lyræ to be added 6

Sun's long. at the time of the transit of α Lyræ. = 8 5 4

Annual number for aberration obs. 1812, p. 251 = 6 5 26

Diff. taken so that it may be less than 6 signs .. = 1 29 38

Log. cosine .... = 9·7037

Gr. obs. 1812, p. 251 + log. max. of aberration. = 1·2523

Log. of aberration .... = 0·9560

Aberration = − 9·04″. The sign is negative, because the difference between the sun's longitude and the annual number for aberration is less than 3 signs. This sign is to be applied to reduce the mean N. P. D. to the apparent.

Equation for Lunar Nutation.

Right ascension of α Lyræ in space .... = 9^s 7° 44′

Longitude of the moon's node .... = 10 29 35

Right ascension − long. moon's node .... = 10 8 9

Right ascension + long. moon's node ...... = 8 7 19

Greenw. obs. 1812, p. 240. $\begin{cases} 8{\cdot}34'' \sin.(AR - \text{long. ☽'s node}) = - 6{\cdot}56'' \\ 1{\cdot}20 \ \sin.(AR + \text{long. ☽'s node}) = - 1{\cdot}10 \end{cases}$

Lunar nutation = − 7·66″. This sign is to be applied to reduce the apparent to the mean N. P. D.

Equation for Solar Nutation.

Gr. obs. 1813, p. 260, col. α Lyræ equation for Nov. 26 = −0·22″

Dec. 6 = −0·37

Hence diff. for 10 days .... = + 0·15″
And diff. for 1 day .... = + 0·015

The equation, therefore, for Nov. 27 = − 0·235″. The sign is to reduce mean to apparent N. P. D.

Equation for Variation.

Gr. obs. 1813, p. 260, equation for Nov. 26. ...... = + 2·71″
Dec. 6. ...... = + 2·78

Hence diff. for 10 days .... = + 0·07″
And diff. for 1 day .... = 0·007

The equation, therefore, for Nov. 27 = + 2·717″. This sign is to reduce the observed N. P. D. to the mean.

Hence the correction for N. P. D. is + 13.05″ + 9·04″ − 7·66″ + 0·235″ + 2·717″ = + 17·382″.

Observed N. P. D. .... =	51°	21′	33·9″
Correction .... =			+ 17·382
Apparent N.P.D. .... =	51	21	51·282
Mean N. P. D. Jan. 1 ..... =	51	22	37·2
Index error .... =			− 45·918

On inspecting these calculations, remembering that they alone refer to observations of a single star on a particular day, and aware that similar are needed for *every* zero star, as often as it is observed, few I apprehend will be found to dispute the point, that corrections in right ascension, and north polar distance, cannot be procured at the *time of wanting them*, without frequent error, and disgusting labour.

Proceed we now to investigate the nature, and extent of the advantages, which would result, were we but supplied annually with a publication containing the daily corrections of the 46 zero stars. These would be comprised, in the equality upon which it would place the British, with the foreign observer—in the inducements which it would hold out, to the study of practical astronomy—in the comparative ease with which it would enable astronomers employed on scientific missions, to obtain accurate astronomical observations—in the facilities which it would afford us in the examination of published observations—in the superior accuracy with which our most important observations would be reduced; and lastly, in the immense labour which would be saved. We will consider them in order.

1. As to the equality to which it would raise the British with the foreign observer.—On the continent by the *industry* of a Danish astronomer, and by the aid of the *Danish government*, the apparent places of the 46 zero stars for every tenth day of the year, have been published annually some time past; but as the mean places

employed at our Royal Observatory, are *not* therein used; and as the corrections themselves do not *exactly* accord with those obtained from Maskelyne's, or Pond's tables, the work to us is rendered comparatively of little value; added to this also, it has never found its way into this country, till many months of its year have elapsed. When, however, it has arrived, I believe it has been employed by some, on the principle that any assistance was better than none. But let it come as it may, sooner or later, it *ill* accords with the *general scientific spirit of the British government*, to allow its astronomers to feel indebted for any *standard* tables, to the *labour* of any *foreign individual*, or to the *generosity of any nation upon earth.*

As to the inducements which it would hold out to the study of practical astronomy;—that practical astronomy labours under *more* disadvantages, than any other pursuit, few I believe will deny; the expence of procuring instruments, the trouble in using them, the uncertainty through capricious weather, of being able to use them when we wish; the time (namely the night) when *alone* they can be employed to the greatest advantage; the sacrifices of the amusements, either of fancy, or of fashion, which a close attention to their use naturally entails upon us; the recollection, that while the labourers in other departments of science are frequently rewarded by some *brilliant* discovery, we " cast our bread upon the waters, expecting not to see it for many days," are powerful preventives to the general cultivation of practical astronomy; but when in addition to these, it is *felt*, that the pleasures of observing are only preparatory to the labours of computing; *known* that more observations can be procured in one night, than can be reduced during the succeeding day; *remembered* that unreduced observations are of no use;* then I say it is astonishing, not that we have so *few*, but that we have *any* private observers at all; and well do I know, that many good instruments are now lying idle, which if the auxiliary alluded to were afforded, would be immediately employed. But, perhaps, some will say, private observers are unworthy of consideration.† If any such there be, I would remind them, that a private observer at Wanstead, discovered the aberration of light; and but for the labours of a private observer at Westbury, the Greenwich mural quadrant would probably, at the present moment, be distributing its inaccuracies to the astronomers of Europe, and the world.

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* Instances have occurred in which such a mass of unreduced observations has accumulated, that the observer has been induced to abandon further observations; knowing that he had by him already, more than he could expect to reduce during his life; and on a recent occasion, a distinguished astronomer was, under such a feeling, impelled to part with his instrument. Thus has practical astronomy lost one of its most industrious cultivators in the retirement of Mr. Groombridge. It will, however, be some satisfaction to know, that his observations, from 20,000 to 30,000 in number, are at the present time being reduced at the *public expence!*

† That Government think *differently*, is obvious from the preceding note.

As to the comparative ease with which it would enable astronomers employed on scientific missions, to obtain accurate astronomical observations;—greatly to the credit of the British government, it has of late years transmitted to distant parts of the globe, several distinguished men, for various scientific purposes; the period which from some cause or other, they can remain at the same station being generally short, any thing which would enable them to multiply observations, or to confer accuracy on those made, would prove *highly valuable*. At home, surrounded by all the paraphernalia for reducing observations, we have shown that the drudgery is hardly tolerable; how much more severely then must it be felt by the travelling astronomer, whose situation denies him these advantages: his observations are purely differential, having the Greenwich stars as points of departure: grant him, therefore, the daily corrections of them, and his labours, instead of being protracted, would terminate, almost as soon, as the observations are completed.

As to the facilities which it would afford to the examination of published observations;—on this head I would dilate a little; and *much* as I respect the Greenwich observations, I would with becoming deference suggest, that some alterations be made in the printed copies; whereby the invaluable data which they furnish, may more readily be converted to immediate use. For the right ascension observations, I would propose the insertion of a column immediately after that which contains the mean transit, in which should be presented the clock's error by each observed zero star; so would the position of the instrument, with regard to the meridian, be instantly seen, although superior and inferior transits, of the same circumpolar stars, through unpropitious weather, may not have been procured. Were also the clock's daily rate computed from consecutive clock's *errors*, by the same stars in lieu of consecutive *transits* of the same stars, increased accuracy would be the result. For the observations of polar distance, two additional columns would be desirable; one appropriated to the correction for barometer and thermometer, the other wherein the index error, as found by each star, might be entered in the same manner as was hinted at for the clock's error; and at the end of each day's work, the *mean* index error should be inserted, so that a person, wanting the polar distance of any comet, or planet, might easily arrive at it by applying to its observed place corrected for refraction, the necessary equation for index error. If also the apparent diameter of the planet, as observed with the micrometer were subjoined, it would, I think, be an improvement. Observations thus published would, with the assistance of the *daily corrections*, be readily examined, and occasional errors, however trivial, would promptly be discovered; but without such an arrangement, or such an aid, the labour of wading through a mass of observations of zero stars *alone*, is such that few individuals would think of undertaking it. I have dwelt upon this subjec from an opinion (perhaps a foolish one) I entertain, that much the differences between Dr. Brinkley's and Mr. Pond's ca logues, may originate not in the observations themselves, but the manner in which those observations may be reduced.

As to the superior accuracy with which our most importan observations would be reduced;—corrections computed at the tim must *alway* be liable to error; but if taken from tables, the acc racy of which is well established, this source of mischief *must* excluded.

Lastly, as to the labour which the published correction would save;—this, perhaps, it would be difficult precisely ascertain. In a former part of this paper, it has been said, th three minutes would be required for obtaining the corrections a single star, in right ascension only; and I think those of nor polar distance cannot be procured in less than twelve; so th fifteen minutes will be spent in getting both corrections. Now it is not too much to say, that 20 zero stars may be observed daily, in good observing weather; and supposing 90 such days to occur in the year, 450 hours will be consumed in obtaining corrections, which were they published annually, might be appropriated to some useful purpose. This, however, would be the labour saved at *one* observatory only, say at Greenwich; but there are other observatories, where as well as at Greenwich, the advantages would be felt; some dependent upon Government; some belonging to public bodies; and others, the property of private individuals; that we may form something like an estimate, of the aggregate benefit, let us enumerate the principal ones. Connected with Government, we have the observatories at the Cape, at Kew, at Portsmouth, and at Bagshot. Belonging to public bodies, we have the observatories at Madras, at Dublin, and at Oxford. As property of private individuals, the observatories of Col. Beaufoy, and Sir Thomas Brisbane; of the Rev. Mr. Catton, and Mr. Cooper; of the Rev. Mr. Evans, and Major Kater; of the Rev. Dr. Pearson and Mr. Sheepshanks. He, therefore, who can *doubt* that an immensity of labour would be saved by published corrections may be supposed able to doubt almost his own existence.

Now as to the expence of procuring the publication in question. At present the apparent places* of 24 of the Greenwich stars for every 10th day of the year, are published by the Board of Longitude; so that the corrections for only 20,† remain to be purchased. What the Commissioners may pay for those of the 24, I know not; but I well know that daily corrections in right ascension and north polar distance of the 46 stars for one year may be calculated by one computer for forty pounds; it is not likely, therefore, to be far from correct, if we say that for forty pounds more than are at present expended, the daily corrections might be procured, done by two *separate* computers, and consequently entitled to every possible confidence.

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* The corrections are preferable to the apparent places, as they enable the observer to use the mean places from the latest determinations.

† The two stars of *α* Libræ, and the two of *α* Capricorni, require only that the corrections for one star of each should be computed.

Having shown what would be the benefits which would result from such a publication; having pretty well ascertained what would be the expence of procuring it; it only remains to see, how far the former are equivalent to the latter;—the task is easy. To place the British observer on an equal footing with the foreign, is worth *something*;—to induce individuals to the study of practical astronomy, is surely worth *something*;—to enable travelling astronomers to multiply observations, and to prove the accuracy of those they make, is *certainly* worth *something*;—to afford facilities to the examination of published observations, is *doubtless* worth *something*;—to entail accuracy in the reduction of the most important observations, is *unquestionably* worth *something*;—and to save an immensity of labour is *indisputably* worth *something*. What may be the *individual* value of the advantages just enumerated, it is needless to inquire; but taken *collectively*, I hesitate not to say, that it is equivalent at least to *ten times the sum* which need be spent *in procuring it*.

Hence I can come to the conclusion, "*That the daily corrections in right ascension and north polar distance of the 46 new stars, should be published annually at the public expence.*"

Blackman-street, Jan. 29, 1824. **James South.**

Article XIX.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1823. *Part II.*

(*Continued from* p. 65.)

XVI. *An Account of an Apparatus on a peculiar Construction for performing Electromagnetic Experiments.* By W. H. Pepys, Esq. FRS.—(See *Annals*, N. S. v. 392.)

XVII. *On the Condensation of several Gases into Liquids.* By M. Faraday, Chemical Assistant in the Royal Institution. Communicated by Sir H. Davy. (See the present number, p. 89.)

XVIII. *On the Application of Liquids formed by the Condensation of Gases as mechanical Agents.* By Sir Humphry Davy, Bart. Pres. RS.

"The elasticity of vapours in contact with the liquids from which they are produced under high pressures by high temperatures, such as those of alcohol and water," observes the President, after some prefatory remarks, "is known to increase in a much higher ratio than the arithmetical one of the temperature; but the exact law is not yet determined; and the result is a complicated one, and depends upon circumstances which require to be ascertained by experiment. Thus the ratio of the elastic force, dependent upon pressure, is to be combined with that of the expansive force dependent upon temperature; and the greater loss of radiant heat at high temperatures, and the developement of latent heat in compression, and the necessity for its reabsorption in expansion (as the rationale of the subject is at present understood) must awaken some doubts as to the economical results to be obtained by employing the steam of water under very great pressures, and at very elevated temperatures.

"No such doubts, however, can arise with respect to the use of such liquids, as require for their existence even a compression equal to that of the weight of 30 or 40 atmospheres: and where common temperatures, or slight elevations of them, are sufficient to produce an immense elastic force; and when the principal question to be discussed is whether the effect of mechanical motion is to be most easily produced by an increase or diminution of heat by artificial means.

"With the assistance of Mr. Faraday I have made some experiments on this subject, and the results have answered my most sanguine expectations. Sulphuretted hydrogen, which condenses readily at 3° Fahr. under a pressure equal to that which balances the elastic force of an atmosphere compressed to $\frac{1}{14}$, had its elastic force increased so as to equal that of an atmosphere compressed to $\frac{1}{17}$ by an increase of 47° of temperature. Liquid muriatic acid at 3° exerted an elastic force equivalent to that of an atmosphere compressed to $\frac{1}{20}$; by an increase of 22°, it gained an elastic force equivalent to that of an atmosphere compressed to $\frac{1}{25}$; and by a further addition of 26°, an elastic force equivalent to that of air condensed to $\frac{1}{40}$ of its primitive volume. These experiments were made in thick glass tubes hermetically sealed. The degree of pressure was estimated by the change of volume of air confined by mercury in a small graduated gage, and placed in a part of the tube exposed to the atmosphere, and the temperatures were diminished from the degree at which the gage was introduced, i. e. the atmospheric temperature by freezing mixtures; so that the temperature of the air within the gage could not be considerably altered; and as the elastic fluid surrounding the gage must have had a higher temperature than the condensed fluid, the diminution of the elastic force of the vapour from the fluids cannot be considered as overrated.

"From the immense differences between the increase of elastic force in gases under high and low pressures, by similar increments of temperature, there can be no doubt that the denser the vapour, or the more difficult of condensation the gas, the greater will be its power under changes of temperature as a mechanical agent: thus carbonic acid will be much more powerful than muriatic acid. In the only experiment which has been tried upon it, its force was found to be nearly equal to that of air compressed to $\frac{1}{20}$ at 12° F. and of air compressed to $\frac{1}{36}$ at 32°, making an increase equal to the weight of 18 atmospheres by an increase of 20 of temperature; and this immense elastic force of 36 atmospheres being exerted at the freezing point of water.*

"In applying the condensed gases as mechanical agents there will be some difficulty; the materials of the apparatus must be at least as strong and as perfectly joined as those used by Mr. Perkins in his high pressure steam engine: but the small differences of temperature required to produce an elastic force equal to the pressure of many atmospheres, will render the risk of explosion extremely small; and if future experiments should realise the views here developed, the mere difference of temperature between sunshine and shade, and air and water, or the effects of evaporation from a moist surface, will be sufficient to produce results, which have hitherto been obtained only by a great expenditure of fuel.

"I shall conclude this communication by a few general observations arising out of this inquiry.

"There is a simple mode of liquifying the gases, which at first view appears paradoxical, *namely, by the application of heat;* it consists in placing them in one leg of a bent sealed tube confined by mercury, and applying heat to ether, or alcohol, or water, in the other end. In this manner, by the pressure of the vapour of ether I have liquified prussic gas and sulphurous acid gas, the only two on which I have made experiments; and these gases in being reproduced occasioned cold.

"There can be little doubt that these general facts of the condensation of the gases will have many practical applications. They offer easy methods of impregnating liquids with carbonic acid and other gases, without the necessity of common mechanical pressure.

"They afford means of producing great diminutions of temperature, by the rapidity with which large quantities of liquids may be rendered aeriform; and as compression occasions similar effects to cold, in preventing the formation of elastic substances;

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* "Since this paper was read, Mr. Faraday has ascertained that the vapour of ammonia at 32° exerts an elastic force equal to that of an atmosphere compressed to $\frac{1}{5}$; and at 50° to that of an atmosphere compressed to $\frac{10}{65}$: and that the vapour of nitrous oxide at 32° has an elastic force equal to that of an atmosphere compressed to $\frac{1}{44}$; and at 45° to an atmosphere compressed to $\frac{1\cdot0}{51\cdot3}$ nearly."

there is great reason to believe that it may be successfully employed for the preservation of animal and vegetable substances for the purposes of food."

An appendix to this paper gives the following statement:—

"In investigating the laws of the elastic forces exerted by vapours or gases raised from liquids by increase of temperature under compression, one of the most important circumstances to be considered is the rate of the expansion, or what is equivalent, of the elastic force, in atmospheres in different states of density.

"It has been shown by the experiments of MM. Dalton and Gay-Lussac, that elastic fluids of very different specific gravities expand equally by equal increments of temperature, or, as it may be more correctly expressed, according to the elucidations of MM. Dulong and Petit, that mercury and air, or gases, are equivalent in their expansions for any number of degrees in the thermometrical scale between the freezing and boiling points of water; and the early researches of M. Amontons seemed to show that the increase of the spring or elastic force of air by increase of temperature, was in the direct ratio of its density. I am not however acquainted with any direct researches upon the changes of volume produced in gases in very different states of condensation and rarefaction by changes of temperature, and the importance of the inquiry, in relation to the subject of my last communication to the Society, induced me to undertake the following experiments.

"Dry atmospherical air was included in a tube by mercury, and its temperature raised from 32° Fahr. to 212°, and its expansion accurately marked. The same volumes of air, but of double and of more than triple the density under a pressure of 30 and 65 inches of mercury, were treated in the same manner, and in the same tubes; and when the necessary corrections were made for the difference of pressure of the removed column of mercury, it was found that the expansions were exactly the same.

"An apparatus was constructed, in which the expansions of rare air confined by columns of mercury were examined and compared with the expansions of equal volumes of air under common pressure; when it appeared, that for an equal number of degrees of Fahrenheit's scale, and between 32° and 212° they were precisely equal, whether the air was $\frac{1}{2}$, $\frac{1}{3}$, or $\frac{1}{6}$ of its natural density.

"Similar experiments were made, but they were necessarily less precise, with air condensed six and expanded fifteen times, with similar results."

XIX. *On the Temperature at considerable Depths of the Caribbean Sea.* By Capt. Edward Sabine, FRS.: in a Letter addressed to Sir H. Davy.—(See *Annals*, N. S. v. 462.)

XX. *Letter from Capt. Basil Hall, RN. to Capt. Kater, communicating the Details of Experiments made by him and Mr.*

Henry Foster, with an Invariable Pendulum; in London; at the Galapagos Islands in the Pacific Ocean, near the Equator; at San Blas de California on the NW Coast of Mexico; and at Rio de Janeiro in Brazil. With an Appendix, containing the Second Series of Experiments in London, on the Return.

"*Abstract of the most exact Results at each Station.*"

Stations.	Diminution of Gravity from Pole to Equator.	Ellipticity.	Length of Equat. Pend.
Galapagos 0°32′ 0″N	·0051412	$\frac{1}{284{\cdot}98}$	39·017196
San Blas.. 21 30 25 N	·0054611	$\frac{1}{313{\cdot}55}$	39·00904
Rio. ..... 22 55 22 S	·0053431	$\frac{1}{302{\cdot}37}$	39·01206

(*To be continued.*)

ARTICLE XX.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Dec. 1 (*continued*).—When the President had concluded his address to the Astronomer Royal, the Society proceeded to the election of a Council and Officers; and the following were declared duly elected:—

Of the Old Council.—The Right Hon. Sir H. Davy, Bart.; Samuel Goodenough, Lord Bishop of Carlisle; W. T. Brande, Esq.; Taylor Combe, Esq.; J. W. Croker, Esq.; Davies Gilbert, Esq.; Charles Hatchett, Esq.; Sir Everard Home, Bart.; John Pond, Esq.; W. H. Wollaston, MD.; Thomas Young, MD. *For. Sec.*

Of the New Council.—Bernard Edward, Duke of Norfolk; Sir James Mac Gregor, Bart.; William Allen, Esq.; Major Thomas Colby; James Ivory, Esq.; William Marsden, Esq.; W. G. Maton, MD.; Edward Rudge, Esq.; William Sotheby, Esq.; Henry Warburton, Esq.

President.—The Right Hon. Sir H. Davy, Bart.

Treasurer.—Davies Gilbert, Esq.

Secretaries.—W. T. Brande, Esq. and Taylor Combe, Esq.

Dec. 11.—John Bayley, Esq. and George Townley, Esq. were admitted Fellows of the Society; and MM. Fourier and Vauquelin were elected Foreign Members.

A paper was communicated, "On the Nature of the Acid and Saline Matters usually existing in the Stomachs of Animals;" by William Prout, MD. FRS.

The object of this paper was to prove, that the acid usually found to exist in the stomach of animals during the digestive process is the muriatic acid, and that the saline matters consist chiefly of the alkaline muriates.

The method adopted by the author to prove this, was to digest the contents of the stomach of a rabbit, or other animal, in distilled water as long as they imparted any thing to that fluid. The solution was then divided into four equal portions. The first of these was evaporated to dryness in its natural state, and the residuum burnt, by which means the muriatic acid in union with a *fixed* alkali was ascertained. Another portion was supersaturated with potash, evaporated to dryness, and burnt as before, and thus the *total* quantity of muriatic acid present determined. A third portion was exactly neutralized with a solution of potash of known strength, which gave the proportion of *free* acid present. A fourth portion was reserved for miscellaneous experiments. From the results thus obtained, checked by others, the author was enabled to ascertain the proportion of muriatic acid present, whether in union with a fixed or volatile alkali, or in an unsaturated state; and the quantity in the latter state was always found to be considerable, and in some instances greater than the quantity in combination. Dr. Prout obtained similar results in different animals, as well as in the human subject, and in one instance, from 20 ounces of fluid ejected from the human stomach, in a severe derangement of that organ, he found upwards of half a dram of muriatic acid of specific gravity 1·160.

The reading was also commenced of a paper, entitled, "An Inquiry respecting the supposed Heating Effect beyond the Red End of the Spectrum; by Baden Powell, MA. of Oriel College, Oxford:" communicated by J. G. Children, Esq. FRS.

Dec. 18.—The reading of Mr. Powell's Inquiry was concluded.

The primary object was to ascertain whether the explanation of the invisible heating rays suggested by Prof. Leslie, viz. that the effect was owing to a concentration of secondary light reflected from the clouds, refracted to a position just without the red rays, was correct or not.—(Inquiry into the Nature and Propagation of Heat, p. 457, and note 45.)

The experiment was tried with a differential thermometer, having the sentient bulb blackened with Indian ink. The prism was placed in situations most favourable to such an effect, and in various inclinations; but the effect exterior to the red rays was never more than 3°, while in the extreme visible red, it was often from 16° to 25°.

The effect of altering the *coating* was next tried; and on covering the bulb with thin yellowish-brown silk, it stood in the extreme red rays at 7°; half an inch beyond, at 7°; while, when merely blackened, it stood in the red at 12°; half an inch beyond, at 2°.

It appears then that the exterior heating effect is not produced in proportion to the *darkness of colour* in the bulb, but is more developed by a *thicker and rougher* coating, not so sensible *to light*, but more so to simple radiant heat.

A variety of different coatings were then tried, with many of which an exterior effect was produced nearly equal to that in the red rays.

These coatings being described and arranged in the order of the greatest exterior effect relatively to the interior, the general result was, that the magnitude of exterior effect, though doubtless caused in some degree by secondary light refracted to that position, is chiefly attributable to a peculiar heating power which differs in nature from that acting *within* the visible spectrum, in the circumstance of its being more developed by such surfaces as are known to be most sensible to the absorption of *common* non-luminous *radiant heat*, without reference to darkness of colour, or absorptive power for *light*.

Coatings equally described as *black* may be very different in their power of absorbing simple heat, and to some slight difference of this sort in the coatings of the thermometers employed, it is highly probable that much of the discrepancy between different celebrated observers may be attributed.

The exterior effect being shown to be analogous in its properties to simple heat, an attempt was made to ascertain whether it radiated in any specific direction; for this purpose a plate of glass, which stops common radiant heat, was interposed a little below the prism, but produced no diminution of this effect. When it was placed parallel to the red rays, and forming a sort of boundary to them, so as to intercept any effect coming outwards from them, the heating effect was greatly diminished.

From all these experiments, the conclusion seems to be, that the red rays acquire a power of *radiating heat outwards from their own particles.*

In a supplement, the author describes a heating effect exterior to the cone of light formed by a lens, affected like the former by the nature of the surface on which it acts. He also states, that he has investigated other phænomena connected with these, which may probably tend to afford an explanation of many of the relations of light to heat.

A communication was also read, "On the North Polar Distances of the principal Fixed Stars, by J. Brinkley, DD. FRS."

In this paper Dr. Brinkley controverts the statements of Mr. Pond respecting the southern motion of the fixed stars, as given in the Philosophical Transactions for 1823; and altogether denies the validity of that astronomer's conclusions on the subject. He shows that the Greenwich and Dublin Catalogues of 1813 differ merely a few tenths of a second, and those of 1823 still less; and he ascribes the appearance of a southern motion to a slight error in the Greenwich Catalogue of 1813. In endeavouring to prove this, Dr. B. adduces the observations of Bradley in 1728, of Cassini in France in 1740, of Dr. Maskelyne at

Schehallien, of Piazzi at Palermo, of Mudge in England in 1802, and of Lambton in Hindostan in 1805. The Westbury observations, he remarks, differ too much from the Greenwich to be available in this question; and those of Mechain are opposed by others made with better instruments. Mr. Pond has diminished the quantities in Dr. Brinkley's Catalogue, by applying Bradley's refraction, while he leaves M. Bessel's in their original form; and is thus enabled to place his own as means between them: to which mode of correction, however, Dr. Brinkley strongly objects. The question of Parallax, Dr. B. considers, is still quite open for discussion; but its consideration he defers for the present. Various tables are annexed to the paper, in illustration of the statements it contains.

A paper was likewise communicated, "On the Figure requisite to maintain the Equilibrium of a homogeneous fluid Mass that revolves upon an Axis;" by James Ivory, Esq. MA. FRS.

The Society then adjourned over the Christmas vacation, to meet again on Thursday the 8th of January.

Jan. 8.—At this meeting, Anthony M. R. Story, Esq. was admitted a Fellow; and the reading was commenced of a paper, entitled, "Observations on the Positions and Distances of Three Hundred and Eighty Double and Triple Fixed Stars, made in the Years 1821, 1822, and 1823;" by J. F. W. Herschel, Esq. FRS. and J. South, Esq. FRS.

LINNEAN SOCIETY.

The meetings of this Society were resumed on the 4th of November. Among the presents then on the table were specimens of eighty-five species of birds sent from India, by Major-Gen. T. Hardwicke, FRS. and FLS., comprising many rare and several new species; and with them was a curious species of musk rat; and also the head of *Antilope Quadricornis*, the *Chikara* of Bengal, a notice of Gen. Hardwicke's description of which, read before the Society on the 17th of June last, will be found in the *Annals* for the succeeding August.

The following communications were read:

"A Description of the Swallow-tailed Falcon, *Falco furcatus*, Linn. taken near Hawes, in Wensley Dale, Yorkshire, in 1805;" and, "A Description of a Bird, supposed to be the *Rallus pusillus* of Latham, shot at the same place in 1807;" by W. Fothergill, Esq. communicated by Dr. Sims, FLS.

Observations on the Genus *Onchidium* of Buchanan, with a Description of a new Species; by the Rev. Lansdown Guilding, FLS.

In this paper Mr. Guilding gives an improved generic character of *Onchidium;* class *Mollusca:* ord. *Cephala;* div. *Gasteropoda:* "*Corpus oblongum, repens, subtus planum. Penula carnosa pedem totum tegens. Os anticum, longitudinale. Anus posticus, infra. Tentacula duo retractilia. Oculi terminales.*" He enumerates six species, including a new one thus characterized: "O. occidentale, *dorso fusco, atomis brunneis elevatis sparsis, ventre pallido, lateribus livido-maculatis, brachiis apice divisis.*" Found in moist places of the mountainous parts of St. Vincent's.

Nov. 18.—The reading was commenced of a paper, entitled, "Experiments and Observations on the Light and Luminous Matter of the *Lampyris noctiluca*, or Glow-worm;" by John Murray, FLS.

Dec. 2.—The reading of Mr. Murray's paper was continued; and the following communication was also read:

"Descriptions of nine new Species of the Genus *Carex*, Natives of the Himalaya Alps in Upper Nepal;" by Mr. David Don, Librarian to the Linnean Society.

These Carices were sent to A. B. Lambert, Esq. VPLS. by Dr. Wallich; they bear a greater resemblance to the European than to the American species. Mr. Don, in describing them, has taken for his model the Bishop of Carlisle's Monograph of the British species, in vol. xi. of the Linnean Transactions; their specific characters are as follows:

1. C. *nubigena*, digyna; spiculis subnovenis ovatis confertis, arillis ovatis striatis rostratis bifidis margine denticulato-scabris, glumis ovatis acuminatis, culmo striato nudo infernè tereti, foliis involutis.—2. C. *foliosa*, digyna; spicâ elongatâ, spiculis ovato-oblongis adpressis; inferioribus subremotis, arillis ellipticis rostratis bifidis margine lævibus, glumis ovatis aristatis, culmo acute triquetro scabro, foliis planis.—3. C. *flexilis*, digyna; vaginis elongatis pedunculo brevioribus, spicis filiformibus cernuis apice masculis, glumis ellipticis acutis, arillis ovatis striatis pilosis rostratis.—4. C. *macrolepis*, digyna; vaginis elongatis pedunculo brevioribus, spicis strictis cylindraceis apice masculis, glumis lanceolatis longicuspidatis, arillis ovatis rostratis scaberrimis, costatis apice bipartitis.—5. C. *longipes*, digyna; vaginis elongatis pedunculo 4-plo brevioribus, spicis cylindraceis erectis apice masculis, glumis ellipticis aristatis, arillis ovatis costatis, glabris rostratis.—6. C. *aristata*, trigyna; vaginis elongatis sulcatis, spicis cylindraceis strictis apice masculis; terminalibus omnino masculis, glumis late ellipticis aristatis, arillis ovalibus triquetris rostratis scabris.—7. C. *chlorostachya*, trigyna; vaginis nullis, spicis fœmineis cylindraceis erectis pedunculatis; masculis solitariis, glumis ovato-lanceolatis acuminatis apice scabris, arillis ventricosis costatis apice rostratis bifurcis glumâ longioribus.—8. C. *lenticularis*, digyna; vaginis nullis, spicis fœmineis filiformibus pedunculis patulis; masculis solitariis pedunculatis, glumis cuneatis: acumine longo spinuloso, arillis cuneato-orbiculatis papilloso-micantibus compressis marginatis.—9. C. *alopecuroides*, trigyna; vaginis nullis, spicis fœmineis erectis cylindraceis subsessilibus; masculis solitariis, glumis ellipticis acuminatis, superne scabris, arillis lanceolatis compressis lævibus apice truncatis emarginatis.

ASTRONOMICAL SOCIETY.

Nov. 14, 1823.—This Society held its first meeting after the late recess this evening, when a paper by G. Dollond, Esq. was read, descriptive of a new instrument of his invention for measuring vertical and horizontal angles in practical astronomy. The instrument was exhibited in the room, and consists of two telescopes, two vertical divided circles, and an artificial horizon, by means of which the object is seen by direct and reflected vision; and no less than 32 distinct readings may be obtained of each observation, without the usual attention to levelling the instrument.

An elegant inkstand was presented for the Society's table, and many other valuable presents were received.

The honorary medals we before alluded to (*Annals*, N. S. vi.) were officially announced from the chair this evening; viz. the Society's gold medals, to Charles Babbage, Esq. for his valuable invention of applying machinery to the purposes of calculation, and to Prof. Encke for his investigations relative to the comet which bears his name. The silver medals of the Society, to Mr. Rumker for the re-discovery of M. Encke's comet in 1822; and to M. Pons for the discovery of two comets in the same year, as well as for his great zeal and perseverance in cometary astronomy. These medals are to be presented at the ensuing anniversary of the Society in the present February.

Dec. 12.—The papers read this evening consisted of a very able and elaborate preface which had been prepared by J. F. W. Herschel, Esq. the Foreign Secretary, at the request of the Council, to a series of tables for calculating the places of the principal fixed stars, which have been computed by order of the Society; and will be printed in the forthcoming volume of its memoirs, and a supplement to a paper before read on the theory of astronomical instruments, by Benj. Gompertz, Esq. FRS. and MAS.

Jan. 9.—The papers read at the meeting of this evening were as follows:

Observations on the Comet of 1811 taken at the Havannah, by Don Joseph Joachim de Ferror of Cadiz, deceased, communicated by the President.

These observations were accompanied by computations of the comet in an elliptic orbit, and the elements are very nearly the same as those brought out by M. Argelander.

On the Constants of Deviation occurring in the Reduction of Astronomical Observations, by Benj. Gompertz, Esq. FRS. and MAS.

This paper examines the causes of deviation, and proposes formulæ for their more easy reduction; it is, however, so purely mathematical as not to admit of abridgment within our limits.

On the Opposition of the Planet Mars, which will take place on the 24th of March next; by Francis Baily, Esq. FRS. and VP. Ast. Soc.

This paper we have obtained permission to print at full length; see p. 107 of our present number.

GEOLOGICAL SOCIETY.

Nov. 7.—A letter was read, dated May 10, 1823, from George Cumberland, Esq. Hon. Mem. GS. "On a Fossil of the Chalk," accompanied by a drawing.

A letter was read, dated July 14, 1823, from George Cumberland, Esq. Hon. Mem. GS. "On a new Species of Encrinus found in the Mountain Limestone, near Bristol."

A notice was read, containing an Analysis of the Aluminite of St. Helena, by Dr. Wilkinson, of Bath: communicated by Col. Wilks, MGS.

On this analysis Col. Wilks observes, that there is a remarkable difference between the component parts of the aluminite of St. Helena, and the sub-sulphate of alumine found at Newhaven and Halle, as given by Phillips, p. 111.

A paper was read on the Geology of Parts of the Islands of Madeira, Porto Santo and Baxo, by T. E. Bowdich, Esq.

From the investigations of Mr. Bowdich, it appears that such parts of these islands as he examined consist principally of horizontal strata of limestone and sandstone containing fossils, intersected, and sometimes capped by basalt.

Nov. 21.—An extract of a letter was read from the Rev. Lansdown Guilding, MGS. containing an Account of a Fossil found in the Blue Lias at the Berkeley Canal, near Gloucester, accompanied by the Fossil.

A paper was read, "On the Lias of the Coast in the Vicinity of Lyme Regis, Dorset," by H. T. de la Beche, Esq. FRS. FLS. and MGS.

In a former communication in the first part of vol. i. second series, of the Society's Transactions, the author had presented an outline of the geological features of the coast near Lyme Regis. The present paper is intended as supplementary, and the sections before published are referred to. Mr. de la Beche now enters into a detailed description, illustrated by a drawing, of the various strata composing the lias formation.

This formation consists of about 110 feet of lias, composed of more than 72 beds of limestones alternating with the same number of marl beds, surmounted by about 500 feet of lias marls. An account is subjoined of the various fossil shells and other organic remains found in the lias, accompanied with several descriptive drawings.

Dec. 5.—A paper was read, entitled "Remarks on the Geology of Siam and Cochin China, and certain Islands in the Indian Archipelago, and Parts of the adjacent Continent," by John Crawford, Esq. MGS.

METEOROLOGICAL SOCIETY.

The second general meeting of this new and promising association was held on Wednesday, Nov. 12, 1823, in pursuance of the resolutions agreed to at its establishment, as given in the *Annals* for Nov. and was very numerously and respectably attended.

Dr. Birkbeck having been called to the chair, the business of the evening commenced by the admission of a number of new members, among whom were W. Allen, Esq. FRS.; W. H. Pepys, Esq. FRS.; B. C. Brodie, Esq. FRS.; H. T. De la Beche, Esq. FRS.; G. M. Paterson, MD. M. Asiat. Soc. Calcutta, &c.

The Society then proceeded to the election of a Council and Officers; when the following gentlemen were appointed:—

Council.—John Bostock, MD. FRS.; Thomas Forster, MB. FLS.; William Shearman, MD.; C. J. Roberts, MD.; Luke Howard, Esq. FRS.; J. F. Daniell, Esq. FRS.; Richard Taylor, Esq. FLS.; E. W. Brayley, Jun. Esq.

President.—George Birkbeck, MD. M. Ast. Soc. MGS. &c.

Treasurer.—Henry Clutterbuck, MD.

A provisional draught of regulations for the government of the Society having been read, it was referred to a committee for revision, and the Society then adjourned.

On the 14th of Jan. 1824, a third general meeting took place, at which the Code of Regulations, as revised by the committee, was adopted by the Society; and the meeting being then resolved into an ordinary one, a Report from a Committee was read, on the objects to which the attention of the Society should primarily be directed; together with a paper on the Vernal Winds, by John Gough, Esq. of Kendal, and several other communications; all which we hope to notice more particularly in our next.

Article XXI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Dark and bright Lines traversing the Spectrum.*

(To the Editor of the *Annals of Philosophy.*)

SIR, *Jan.* 14, 1824.

In your number for August, 1822, a short notice appears respecting the prismatic experiments of M. Frauenhofer, a full account of which is given in the Edinburgh Philosophical Journal, No. 18, p. 288, and continued in the subsequent number. You will, perhaps, allow me to trespass upon a small space in the next number of the *Annals* in order to make a remark relative to the subject of the papers alluded to, which I am induced to do from not finding it done from higher quarters. The observation I wish to make is this, that although we must allow M. Frauenhofer full credit for having first minutely described the position of various dark and bright lines traversing the spectrum, and having employed them as definite points for measuring the dispersion, *yet the fact of the existence of such lines* has been for some years known, though not, perhaps, so generally as it deserves to be. I beg to refer such of your readers as may not be acquainted with it, to a paper by Dr. Wollaston in the Phil. Trans. for 1802, p. 378, in which the existence of such lines under particular circumstances is described as observed by the author. Another observation connected with the same phenomena will be found in p. 380 of the same paper; and in the same volume, p. 395, in a paper by Dr. Young, an important observation is made, bearing on the theory of the phenomena.

I am, yours, &c. B. R.

II. *Analysis of Cleavelandite from Finland.*

M. F. Tengstrom finds this substance to be composed of

Silica	67·99
Alumina	19·61
Soda	11·12
Lime	0·66
Oxide of manganese	0·47
Oxide of iron	0·23
	100·08

III. *Copper Pyrites of Orijarva.*

M. V. Hartwall, of Abo, has analyzed the copper pyrites of Orijarva, with the following results:

Sulphur	36·33
Copper	32·20
Iron	30·03
Silica	0·99
Oxide of mang. and earthy matter.	1·30
	100·79

The author of this analysis remarks, that its results nearly agree with those given of the copper pyrites of Cornwall, given in the *Annals*, vol. iii. N. S. p. 300.

IV. *Scapolite from Pargas.*

The last named chemist has also given an analysis of a new species of scapolite from Pargas: it consists of

Silica	49·42
Alumina	25·41
Lime	15·59
Soda	6·05
Oxide of iron	1·40
Magnesia	0·68
Oxide of manganese	0·07
Loss by heat	1·45
	100·07

VII. *Manufacture of Pianoforte Wire.*

(To the Editor of the *Annals of Philosophy.*)

SIR, *London, Oct.* 8, 1823.

I should feel much obliged to you, or any of your correspondents, for the communication of any particulars respecting the manufacture of pianoforte wire, and the cause of the very decided superiority of what is called Berlin steel wire over all that is made in England. Have any of the new alloys of steel and silver, &c. or has Mr. Brookedon's method of drawing through conical holes made in diamonds, rubies, sapphires, or other hard stones, been tried, to make pianoforte wire? Considering the great quantity and value of the wire used in musical instruments, made in the greatest perfection in this country, it is to be lamented that our wire-drawers suffer themselves to be excelled by foreigners.

What mode of whitening brass wire will best preserve it from oxidisement, and least injure its elasticity?

I am, Sir, your constant reader, M. A.

Article XXII.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Dr. Uwins will shortly publish a Compendium of Medical Theory and Practice, founded on Cullen's Nosology. 12mo.

Thomas Sandwith, Esq. has in the press an Introduction to Anatomy and Physiology; in a duodecimo volume.

James Buckingham, Esq. Author of 'Travels in Palestine,' will shortly publish 'Travels among the Arab Tribes inhabiting the Countries East of Syria.'

An Account of the Life and Writings of the late Thomas Brown, MD. Professor of Moral Philosophy, Edinburgh; by the Rev. David Welsh.

JUST PUBLISHED.

The Westminster Review, No. I. 6*s*.

A second edition of Mr. Tredgold's Essay on the Strength of Cast Iron and other Metals, much augmented, particularly in the Experimental Parts, and the Illustration of the Application by popular Examples.

Pathological Observations; Part I.—On Dropsy, Purpura, and the Influenza of the latter end of the year 1822, and beginning of 1823, &c. By W. Stoker, MD. Senior Physician to the Fever Hospital, Dublin. 8vo. 8*s*.

Edinburgh Medical and Surgical Journal. No. I. New Series. 8vo. 6*s*.

An Essay on the Inventions and Customs of Ancient and Modern Nations in the Use of Wine and other Liquors; with an Historical View of the Practice of Distillation. By Sam. Morewood. 8vo. 12*s*.

The Encyclopædia Metropolitana, Part XI. containing Magnetism and Electro-Magnetism, &c. 1*l.* 1*s.*

A Tour through the Upper Provinces of Hindostan, between the years 1804 and 1814; with Remarks and authentic Anecdotes; to which is annexed a Guide up the River Ganges: with a Map of the Ganges. 8vo. 9*s.*

A new Edition of Prof. Buckland's Reliquiæ Diluvianæ, attesting the Action of a Universal Deluge: with 27 Plates. 4to. 1*l.* 11*s.* 6*d.*

Article XXIII.

NEW PATENTS.

T. Hopper, Esq. Reading, Berkshire, for certain improvements in the manufacture of silk hats.—Nov. 20.

C. A. Deane, Charles-street, Deptford, Kent, ship-caulker, for his apparatus or machine to be worn by persons entering rooms or other places filled with smoke or other vapour, for the purpose of extinguishing fire, or extricating persons or property therein.—Nov. 20.

J. Perkins, Hill-street, London, and J. Martineau the younger, City Road, Middlesex, engineers, for their improvement in the construction of the furnace of steam-boilers and other vessels, by which fuel is economized, and the smoke is consumed.—Nov. 20.

J. Bourne, Denby, Derbyshire, stone-bottle manufacturer, for certain improvements in the burning of stone and brown ware in kilns or ovens, by carrying up the heat and flame from the furnace or fire below to the middle and upper parts of the kiln or oven, either by means of flues or chimneys in the sides thereof, or by moveable pipes or conductors to be placed within such kilns or ovens; also by conveying the flame of one kiln more into others by means of chimneys, and thus permitting the draft and smoke of several kilns or ovens to escape, whereby the heat is increased, and the smoke diminished.—Nov. 22.

J. Slater, Saddleworth, Yorkshire, clothier, for improvements in the machinery to facilitate the operation of cutting or grinding wool or cotton from off the surfaces of woollen cloths, kerseymeres, cotton cloths, or mixtures of the said substances, and for taking or removing hair or fur from skins.—Nov. 22.

T. Todd, Swansea, South Wales, organ-builder, for his improvement in producing tone upon musical instruments of various descriptions.—Nov. 22.

S. Brown, Gent. Windmill-street, Lambeth, Surry, for his engine or instrument for effecting a vacuum, and thus producing powers by which water may be raised and machinery put in motion.—Dec. 4.

A. Buchanan, Catrine Cotton Works, for a certain improvement in machinery heretofore employed in spinning-mills in the carding of cotton and other wool, whereby the top cards are regularly stripped and kept clean by the operation of the machinery without the agency of hard labour.—Dec. 4.

J. Parkes, Manchester, civil engineer, for a certain method of manufacturing salt.—Dec. 4.

Article XXIV.

METEOROLOGICAL TABLE.

1823.	Wind.	Barometer.		Thermometer.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
12thMon.								
Dec. 1	W	29·90	29·69	52	39	—	—	
2	S W	29·69	29·69	52	36	—	28	
3	S W	29·69	29·45	54	39	—	10	
4	W	29·93	29·50	47	30	—		
5	W	29·93	29·62	46	32	—	41	
6	N E	30·59	29·93	42	28	—	04	
7	W	30·59	30·55	36	31	—		
8	W	30·55	30·46	44	34	·32		
9	N W	30·51	30·44	44	27	—		
10	S W	30·51	30·28	44	29	—		
11	S W	30·28	29·97	48	40	—		
12	W	30·08	29·91	43	33	—		
13	N W	30·34	30·03	40	26	—		
14	N W	30·33	30·29	41	31	—		
15	N W	30·33	30·31	44	31	—		
16	S W	30·31	29·82	47	39	—	—	
17	S E	29·82	29·10	48	32	—	38	
18	W	29·75	29·60	40	30	—		
19	N W	29·75	29·53	36	26	·45		
20	S E	29·53	29·36	45	32	—	10	
21	N W	29·76	29·36	50	36	—	—	
22	N W	29·93	29·76	45	30	—	—	
23	S W	30·07	29·93	45	36	—	26	
24	S W	30·10	30·07	48	42	—	20	
25	S W	30·10	29·97	49	39	—	07	
26	S W	29·97	29·39	46	39	—	10	
27	S W	29·62	29·39	45	35	—		
28	S W	29·62	29·37	52	39	—	06	
29	S W	29·37	29·27	46	39	—	06	
30	S W	29·58	29·27	45	38	—	02	
31	W	29·65	29·46	49	38	·35	25	
		30·59	29·10	54	26	1·12	2·33	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Twelfth Month.—1. Cloudy. 2. Rainy. 3. Cloudy and fine: a furious gale from the W all night. 4. Fine: wind still very high. 5. Foggy morning: cloudy. 6. Rainy morning: cloudy. 7. Foggy. 8. Foggy morning: fine. 9. Fine. 10. Cloudy. 11, 12. Fine. 13. Fine: bleak. 14. Fine clear morning: day very fine: evening foggy. 15. Very fine. 16. Overcast. 17. Rainy: a strong gale of wind in the evening, accompanied by an uncommonly rapid depression of the barometer. 18. Fine. 19. Foggy: gloomy. 20. Overcast: drizzling. 21. Foggy morning: afternoon fine. 22. Gloomy. 23, 24. Rainy: gloomy. 25. Drizzling. 26. Gloomy. 27. Fine. 28. Drizzling: night stormy. 29. Fine: some rain at night. 30. Cloudy and fine. 31. Fine day: night windy, with rain.

RESULTS.

Winds: NE, 1; SE, 2; SW, 13; W, 8; NW, 7.

Barometer:	Mean height	
	For the month	29·885 inches.
	For the lunar period, ending the 24th	
	For 15 days, ending the 10th (moon south)	
	For 13 days, ending the 23d (moon north)	
Thermometer:	Mean height	
	For the month	39·822°
	For the lunar period	
	For 30 days, the sun in Sagittarius	
Evaporation		1·12 in.
Rain		2·33

Laboratory, Stratford, First Month, 22, 1824. R. HOWARD.

Note.—Some doubt having arisen as to the accuracy of the *high mean* lately assigned to the barometrical observations in this Register, we have thought it needful to verify the actual height of the barometer employed, by comparison with a good standard.

On the 16th inst. at three, p. m. I suspended by the side of the barometer in question, the one belonging to my friend J. F. Daniell, described in page 358 of his "Meteorological Essays," and which he considers (with due allowance for the depression of the column, caused by the smaller diameter of the tube) to agree with the excellent standard barometer, lately constructed under his direction for the Royal Society. The result was very satisfactory: the mountain barometer, and that employed for this Register, stood respectively at 30·66 in.; nor could a difference of 1-100th of an inch be found between them, by either of two observers, who examined them at intervals during a full hour. The temperature of each was 55°: the inner diameter of the mountain barometer (the quicksilver of which has been boiled *in the tube*), is 0·15 in.: the inner diameter of the wheel barometer, as nearly as could be ascertained, 0·23 in. The quicksilver has not been boiled in this instrument. The barometer was very nearly stationary on the 16th, from noon to midnight.

In a comparison made this day, for an hour before noon, in the lower part of the scale, the instruments at the conclusion stood thus, at temp. 58°.

The siphon barometer	28·94
The mountain ditto	28·895
Difference	·045

The correction for the capacity of the cistern in the mountain barometer being applied *reduces* the difference in the present case to ·014, and *creates* a difference in the former of ·011 in. both in excess on the part of the siphon barometer. The corrections for temperature and capillary depression cannot here be so accurately applied, on account of the different construction of the two instruments. On the whole, there appears no ground to disturb the adjustment of our own barometer.

Tottenham, First Month, 23, 1824. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

MARCH, 1824.

ARTICLE I.

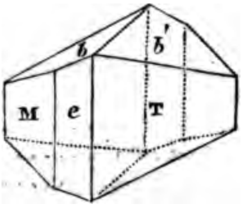
On the Crystalline Forms of Artificial Salts.

By H. J. Brooke, Esq. FRS.

(*Continued from* p. 117.)

Tartrate of Potash.

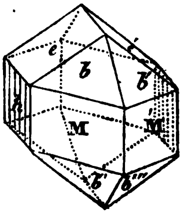
The primary form is a *right oblique angled prism,* with cleavages parallel to the lateral planes.



M on T	89°	30′
M on *e*	142	13
M on *b*	107	30
T on *e*	127	17
T on *b′*	103	40

Bitartrate of Potash.

It has been already observed by Dr. Wollaston, that the cleavages of the crystals of this salt are parallel to the lateral planes and diagonals of either a rectangular or a rhombic prism. From the figures and measurement of several crystals, I am induced to adopt *the right rhombic prism* as the primary form. The annexed figure represents the planes which occur on many of the crystals placed *symmetrically* in relation to the primary planes.



M on M′	107°	30′
M on *h*	126	15
M on *b*	117	2
c on *h*	125	30
c on *c′*	109	0
b on *b‴*	77	0

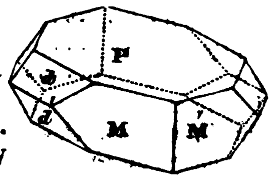
There is a bright cleavage parallel to the edge between M and M′ and perpendicular to *h*, and consequently parallel to the great diagonals of the primary terminal planes. The crystals may be also cleaved parallel to *h*, and to M and M′. The figure, it has been observed, is *symmetrically* drawn, but the crystals are frequently much distorted by the disproportionate extension of some planes and the disappearance of others; so that it is not always easy to compare them with any general type of the whole. We may, however, be much assisted in this comparison by the character of the plane *h*, which, in all the crystals I have seen, is *striated*, as shown in the figure. By the assistance of this plane, and the bright cleavage plane perpendicular to it, and by measurement, we may be enabled to compare the crystals with the engraved figure, however irregularly formed they may be. On very many of them the planes *b* and *b‴* are so much enlarged as nearly, if not entirely, to exclude the other four planes which appear in *front* of the figure; while at the *back*, the planes parallel to *b′* and *b″* are similarly enlarged. If these four planes were so much extended as to exclude all the others which appear on the figure, an irregular tetrahedron would result.

On some crystals there is one and sometimes two planes replacing the edge between *h* and M, one of them measuring with *h* about 156°, and the other about 145½°.

Nitrate of Silver.

Primary form *a right rhombic prism.*

P on *d*	116°	36′
M on *d*............	148	0
M on M′	129	31
d on *d′*	126	48



On some crystals received from Mr. Teschemacher the planes *d* were barely visible; while on others from Mr. Cooper, those planes encroached so much on M and M′ as to leave only minute portions of these visible.

ARTICLE II.

Observations and Experiments on the daily Variation of the Horizontal and Dipping Needles under a reduced directive Power. By Peter Barlow, Esq. FRS, of the Royal Military Academy.*

It is now just a century since Mr. Graham discovered the daily change in the variation of the horizontal needle, subsequent to which time numerous observations have been made on the same subject by Wargentin, Canton, Gilpin, Col. Beaufoy, and others, which have all confirmed, with certain shades of variety, the general fact as first described by the ingenious philosopher above named.

The actual daily change, however, is so small, even in the horizontal needle, that it can only be detected with the most careful observations and with the most delicate instruments; and in the dipping needle that change, if any, is so extremely minute, as hitherto to have escaped observation; for it was only in the year 1820, that the Royal Academy of Sciences of Copenhagen proposed the determination of this motion, on satisfactory experiments, as the prize subject for that year; but the prize, I understand, has never been adjudged, no satisfactory communication having been received.

Under this difficulty of observation, it occurred to me, that it would be possible to increase this deviation on both needles, so as to render it distinctly observable, by reducing the directive power of the needle by means of one or two magnets, properly disposed to mask, at least in part, the terrestrial influence; a method which has been long practised by mineralogists and others, when the object has been to detect minute attractions I expected by this means that the cause, whatever it might be, that produces the daily variation, would exhibit itself in an increased degree, and thereby render the results more perspicuous, and fix with more precision than has hitherto been done, the time of change and moment of maximum effect.

Suppose, for example, that a finely suspended horizontal needle, under the natural influence of the earth, makes one vibration in 2″, and that by masking the terrestrial influence by magnets properly adjusted, that time of vibration is increased to 8″; then it would follow that the directive power was reduced to one-sixteenth of the former, and consequently, that any lateral magnetic force acting upon the needle would produce an effect sixteen times greater than before; so that if the former were 12′, the new effect or deviation might be expected to amount to between three and four degrees, and therefore be such as to admit of distinct and satisfactory observation.

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* Abstracted from the Phil. Trans. for 1823, Part II.

A course of experiments carried on for a few days, convinced me that my ideas were correct, and that we might, while the needle was kept in its natural meridian, or rather adjusted to that direction, produce a daily variation to almost any amount. I obtained, for instance, the first day, a maximum deviation of 3° 40′; the second, I increased it by bringing up my magnets to 7°; the third day I reduced it to 2°, and so on. I found also that a very considerable daily change would exhibit itself with the north end held to the south, to the east, west, and, in short, in any position at pleasure, at least within certain limits, which will be pointed out as we proceed.

For this it is only necessary, first, to deflect the needle by repulsion into any required position, and then, by means of another magnet, to modify its directive power, in the same way as when in its natural meridian. Or the same may be done by bringing two magnets with their contrary poles pointing inwards, and each opposite to the pole of the same name of the needle placed between them, and by a slight adjustment of the former to produce the deviation in question; or, which is perhaps still better, the opposing magnets may be brought into the actual direction of the dip, and then adjusted to produce the deflection required.

Having mentioned my ideas and first experiments to my colleague, Mr. Christie, and having expressed a wish that he would repeat them for the sake of verification, he very readily agreed to undertake a complete set, with the needle in its natural meridian, by means of a very delicate compass, and an apparatus he had employed for other experiments, and which admitted of his bringing his neutralizing magnets very exactly into the line of the dip. In the mean time I proposed to undertake the observations on the dipping needle, and on the horizontal needle in different directions; viz. with its north end pointing to the south, east, west, &c. Having, however, met with some embarrassment in the commencement, and having employed, in consequence, a longer time in the observations than I had anticipated, Mr. Christie, after having finished his observation in the meridian, continued them at other points, and has thereby detected several curious and minute peculiarities, which, with his other experiments, will, I hope, accompany this memoir.*

Account of the Observations made on the daily Variation of the Horizontal Needle in various Directions.

My first experiments, as I have already stated, were only matters of trial, from which I had merely ascertained that the idea I had formed was practicable, and that in certain situations the needle had certain directions of motion, but I had obtained no numerical results. Having, however, provided myself with a needle proper for the purpose, very delicate and light, and eight inches and a half in length, I began, towards the end of March, to register the amount of the daily change at every hour, or half hour, from morning to night; my son taking the observations during my occasional absence.

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* Mr. Christie has detailed his experiments in an extended paper, which succeeds Mr. Barlow's present communication in the Phil. Trans.

My first observation in the new series was made with the north end of the needle pointing to the west, balanced in that position with two magnets placed to the southward attracting each extremity; the directive power was considerably reduced, and I obtained a maximum deviation of 3° 15′; which happened at about eleven o'clock in the forenoon, and from which time the variation decreased to a late hour in the evening. The needle was kept in this position for three days, with some change of directive power, but the character of the results, as to the direction of motion, the times of commencement and maximum, &c. were of precisely the same nature, but the amount was more or less, according to the directive power left upon the needle.

Having, however, after a few days, removed my apparatus from the room in which the experiments had hitherto been made, into a bower in my garden, and having detected a remarkable difference in the results obtained in these two situations, I determined to commence the experiments *de novo* in this latter spot, which was at least thirty yards distant from any building; and afterwards to examine the cause of the difference in question. This examination is reported in the conclusion of this article.

[Mr. Barlow here gives a series of tables of observed daily variations, with the north end of the needle directed to the following points of the compass respectively:—north, south, north-east, south-west, east, west, south-east, north-west, north north-east, south-south-west, east-north-east, west-south-west, east-south-east, west-north-west, south-south-east ½ south, (exact bearing N. 16° W. and S. 16° E.) and north-north-west.]

From the above results, although the experiments were not made under such favourable circumstances as I could wish, we may draw some very curious, if not important conclusions; such, for instance, as the following. That while the north end of the needle is directed to any point from the south to NNW, its motion during the forenoon is towards the left hand (the spectator facing the north end of the needle); advancing therefore to some point between the NNW and north; and while it is directed towards any point between the north and SSE it passes to the right hand, that is still to some point between the north and NNW; the south end of the needle at the same time passing of course to some point between the south and SSE; so that it would seem that there ought to be some direction between those limits, viz. between the N and NNW, and the S and SSE, in which the daily motion is zero, or at least a minimum; but whether this is a fixed direction during the year, or whether it has any vibratory motion as the sun changes its declination, or even during his daily course, is a question which cannot be decided without a much longer course of experiments than those I have here the honour to present.

It is also questionable, whether the direction of this line of no daily variation is the same in different parts of the world; a point on which I hope to obtain some information in the course of the present year. Mr. Forster,* of H. M. S. Griper, having very obligingly undertaken to repeat my experiments at Spitzbergen, during the stay of the vessel at that place for the pendulum experiments; and from which we may hope to derive some interesting deductions, particularly in reference to the influence of the direction of the solar rays; for it is clear from the experiments reported in the preceding table, that the amount of the deviation does not entirely depend upon the moment when the heat of the sun is the greatest, as has been generally imagined; for the time of the maximum deviation varies from eleven o'clock in the morning to four o'clock in the afternoon, according to the direction in which the needle is pointed, and to other circumstances that will be mentioned in the conclusion of this article. Mr. Christie's observations are also of a kind to throw great light on this subject.

Another conclusion, which I think we are justified in drawing from the above experiments, is, that the daily change is not produced by a general deflection of the directive power of the earth, but by an increase and decrease of attraction of some point situated between the north and NNW, or between the south and SSE; for I cannot conceive any other hypotheses that will account for two needles, situated as in these experiments, both approaching and both receding at the same time to and from the line of no daily variation; nor for the total suspension or equivocal vibratory motion of a needle when placed towards this direction.

I am sorry, that not foreseeing at the commencement of my experiments, the length to which I should carry them, I did not, from the first, register the temperature and state of the atmosphere; for from certain notes of this kind made lately, it appears to me that the quantity of daily change depends in a greater degree on the intensity of the solar light, than on the mere temperature of the day; although it is certain, from some recent experiments by Mr. Christie, that the change of temperature of the air, during the day, has a much greater effect upon the intensity of action in the opposing magnets, than I could possibly have imagined.

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* I am already highly indebted to this gentleman for the accurate and satisfactory observations he made during the recent voyage of H. M. S. Conway, under the command of Capt. Basil Hall, on the method I had the honour to propose for correcting the local attraction of vessels; and it is with great pleasure that I find he has been directed by the Admiralty to continue his attention to them in the present voyage of the Griper. My best thanks are also due to Capt. Hall, for the facilities he afforded in the instance abovementioned, and for the judgment with which he selected the most appropriate situations for submitting that method to the test of actual experiment.

On the daily Variation of the Dipping Needle.

Notwithstanding my observations on the daily change of this instrument have not been so successful as those on the horizontal needle, yet it will be proper to say a few words on the subject of the experiments, although I do not intend, in the present instance, to give any numerical results; those I have obtained not being so uniform as I could wish, nor such as to justify their publication.

The instrument I employed was made by Messrs. W. and T. Gilbert: it was remarkably free and accurate, and certainly gave results with greater uniformity than any dipping needle I ever used. The needle was only six inches in length, a quarter of an inch broad, and very thin; it performed in the meridian forty-one vibrations in one hundred seconds, when under the usual terrestrial influence; and when masked and adjusted by two magnets placed in the line of the dip, it made only fifteen vibrations and a half in the same time; the power was therefore reduced about eight times.

It is not necessary to explain here the means that I employed, and the precautions I took to ensure stability; it will be sufficient to observe, that I paid the utmost attention to this essential condition, and that I believe my want of success did not arise from any defect in this part of the process, but from the extreme delicacy of this instrument, and the consequent difficulty in adjusting it when under the influence of the neutralizing magnets. I tried its action for three weeks in the house, but the jarring of doors and other circumstances prevented me from drawing any conclusions. I then removed it to the garden, to a spot well protected by trees and shrubs, and fixed the entire apparatus to my garden wall, which is exactly in the magnetic meridian; and further sheltered the whole in the best way I could from the effects of the wind and weather. Indeed the only inconvenience was that I could not leave the needle out in the night, and could therefore only notice what took place in the day time, and this, as I have said above, was not so uniform as I could have desired.

In general a motion commenced soon after the instrument was adjusted in the morning; but it was not of that gradual and progressive kind which indicated an uniformly increasing or decreasing power, as in the other instrument; it passed, for instance, suddenly from one half or quarter degree, to another more or less, and which sometimes in the course of the day would give a difference in the dip to the amount of a degree and a half, or even more, but I seldom saw in it a tendency to return; although when I vibrated it towards night, it commonly took up its morning position. I made these observations with the needle in various directions, viz. with the face of the instrument to the east, west, north, south, &c. but in every case I obtained the same sort of daily motion. The question, therefore, respecting the law of variation of this instrument, still remains to be submitted to fixed principles, although there can be no longer any doubt that it is subject to a daily change.

On a curious Anomaly observed between the daily Variations in-doors and in the open Air.

I have already mentioned that I was, at the commencement of my experiments, a good deal embarrassed and delayed by certain anomalies which I noticed between the daily changes of the needle made in the house and in the garden. These may be stated shortly as follows. That in certain positions of the needle towards the east and west, the daily motion, although it proceeded with the same determinate uniformity in both cases, yet it took place in different directions; passing in the one instance from the east, or west, towards the south, and in the other towards the north, at the same corresponding hours of the day, the motion in both instances being equally distinct, regular, and progressive.

After carefully examining every circumstance that might be supposed to be the cause of this singular change, I could only imagine three, that seemed in any way likely to account for it.

1. Were the two magnets and the compass needle in the two cases in precisely the same relative situation; and if not, might not the cause lie in this discrepancy?

2. The window of the room was to the northward; was it possible that the light, arriving at the needle in this direction, was the cause of the change?

3. There was an iron stove in the room; could it be that this was subject to a periodic increase and decrease of magnetic power?

In order to examine the first of these cases, I measured very carefully the distance, direction, &c. of the compass and magnets while in the garden, and placed them in precisely the same relative situation in the parlour; still the motion in the two cases was reversed.

To examine the second, it occurred to me that if the direction of the motion depended upon that of the light, the needle ought to be wholly stationary in the dark, or when excluded from the solar rays. I therefore kept my room shut for two days, and only examined the needle by the light of a wax taper; but although there was certainly less motion on those days than usual, yet I could come to no satisfactory conclusion; but I still think that further observations will show that the solar light,* and not the solar heat, is the principal operative agent in producing the daily variation. It remained, however, to examine the third query, which I attempted to do as follows. Having placed the compass in its former situation in the garden, I fixed on one side of it a ten inch howitzer shell, in the same direction with respect to the compass as the stove had in the parlour, and at such a distance that it might produce a sensible deviation in the needle, and which I afterwards adjusted to zero by a slight change in the position of the magnets, thus placing the needle, as I imagined, under similar circumstances in both cases, with respect to local attraction; but, notwithstanding I did in this way actually produce an alteration in the daily motion, changing its maximum from eleven o'clock in the morning to about four o'clock in the afternoon, yet the direction of the motion was the reverse of what it was constantly found to be in-doors; the cause therefore of this perplexing anomaly still remains to be discovered.

It is proper to observe that Mr. Christie, having made some of his observations in-doors, and some in his garden, on two compasses at the same time, found the same reversion of motion in the two cases. His house is a mile distant from mine; he has no stove in the room in which the in-door experiments were made; and the only resemblance of situation is, that his window, like mine, is towards the north. It should be further added, that this confirmation of the singular anomaly in question did not arise from his simply repeating my experiment, but grew naturally out of the particular mode he had adopted to prosecute the inquiry; our experiments, with the exception of the first suggestion, are independent, and, therefore, where they both lead to the same result, they may be considered as confirming the accuracy of each; and where there is any difference, they will at least point out those circumstances which require further investigation.

P. S. The experiments to which I have alluded in p. 166—167, made since this article was written, seem to indicate that this anomaly, as well as the circumstance there mentioned, may be occasioned by the daily varying intensity of the opposing magnets.

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* I am sorry I have not the necessary apparatus for repeating Morichini's experiment on the violet ray; but I would suggest to those who have, that the finest test to which this experiment could be submitted, would be to make use of a needle neutralized as above described, by which the magnetic property of the ray, if it possessed any, could not fail of showing itself.

Article III.

On an Improved Apparatus for the Analysis of Organic Products. By Mr. J. T. Cooper.* (With a Plate.)

An easy and accurate method of determining the ultimate elements of bodies composed of carbon, hydrogen, oxygen, and azote, has been of late years a great desideratum among chemists, as such a variety of contrivances have been suggested by scientific individuals, all of which have their peculiar merits and defects. It is presumed the instrument and method of operating now presented to the Society and the public, if not entirely, may be considered as nearly free from those objections which, in my opinion, may be fairly urged against those heretofore in use. It might, however, be considered ungenerous, was I to take upon me the task of pointing out those defects, I shall therefore content myself by briefly stating in this communication, the class of substances to which it is applicable with a view to determining the proportions of their elements, and a description of the method of operating upon each of them.

As this apparatus seems more particularly calculated than any other for operating on volatile matter, such as the essential oils, camphor, benzoic acid, and a variety of similar substances, I shall in the first place describe the method I have adopted in the analysis of this class of bodies; and when it is considered that I write not for those who are accustomed to the more minute and delicate operations of chemical analysis, but for those who are or may be considered as unacquainted for the most part with this subject, I hope I may not be considered as tedious should I venture to give those directions which to the more matured in science may seem to be unnecessary.

The oxide of copper used in the experiments is best procured from the residuum of verdigris (binacetate of copper), which is or was used to be distilled in glass retorts for the preparation of strong acetic acid. The reason I prefer the oxide of copper prepared by this process over any other is, that it is more likely to be free from impurity than that which is prepared by precipitation from acid solutions. Every one who is in the habit of preparing precipitates knows the difficulty there generally is in freeing considerable quantities of precipitated matter from adhering neutral salts; and as the smallest impurity would in some measure contaminate the result of the analysis, it is a very necessary precaution that the oxide, which is by far the greatest in quantity of any substance that is employed in the operation, should be perfectly pure. Should it however happen that at any

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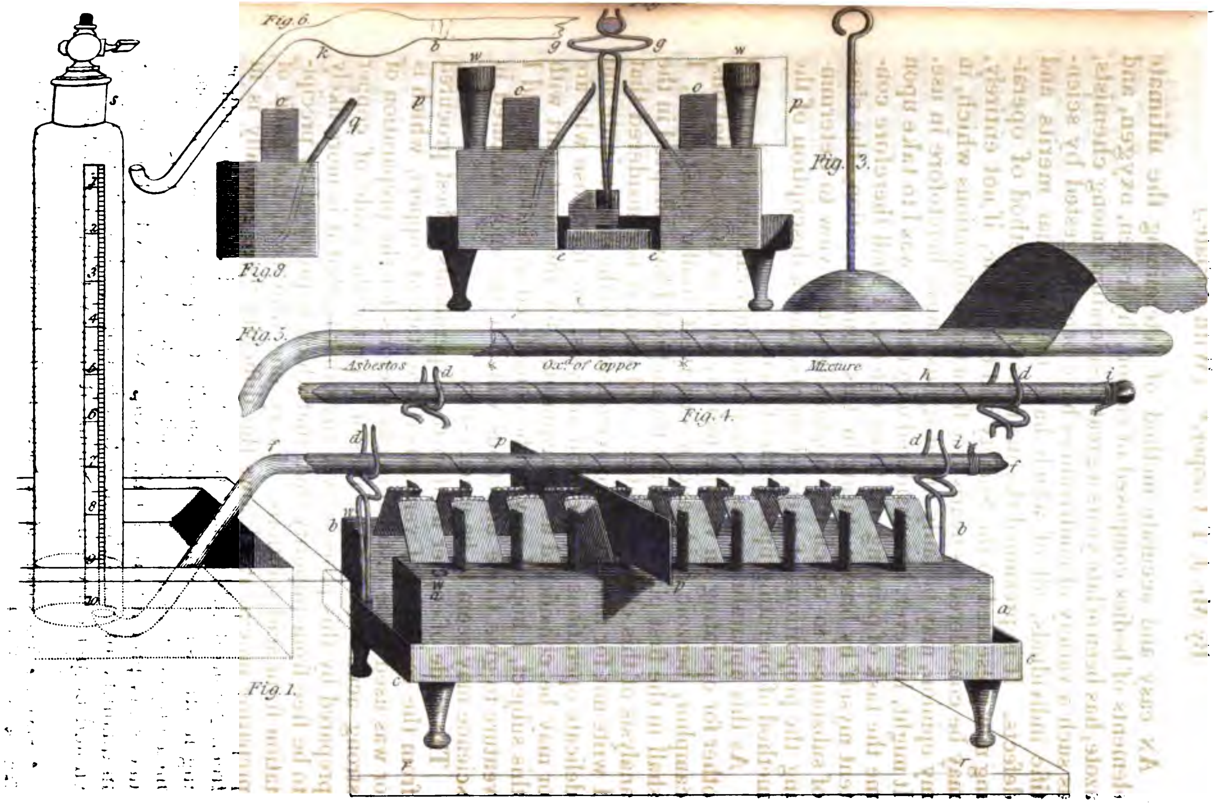
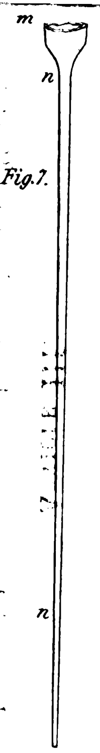
* From the Transactions of the Society for the Encouragement of Arts, Manufactures, &c. Vol. 41.





Engraved for the Annals of Philosophy, for Baldwin, Cradock & Joy March 1. 1824.

time such an oxide is not readily to be procured, the oxide that is obtained by heating copper plate and quenching it in water may be substituted; although I give the decided preference to the former on account of its mechanical texture being much more porous, and consequently exposing a larger surface to the action of substances in vapour passing through it; neither is it so likely to choak up the tube and endanger its bursting, and of course a failure in the experiment. Supposing the residuum above mentioned to be employed, it is requisite to expose it to a red heat for twenty minutes or half an hour to destroy the carbonaceous matter that invariably accompanies it; it should then be pulverised and sifted through a fine wire sieve; that portion which has passed the sieve being again sifted through a fine cyprus or lawn sieve, the finer dust is got rid of, and each of these portions may be separately kept, and is applicable to different purposes.

A tube of hard glass, either of crown or green bottle glass, being selected about 14 or 15 inches long, and from one to two-tenths of an inch internal diameter, clean the inside from dust by passing through it a piece of cotton, then make it as hot from end to end as the fingers can conveniently bear, and draw air through it into the mouth (but not blow through it) while it is still hot, to ensure its perfect freedom from adhering moisture on its inside, and while still warm seal up one end with the blowpipe; the tube may be now balanced, but it is necessary in this, as in all other operations of analysis where very small quantities are concerned, that the beam should be affected by 1-200 or 1-300 of a grain, even when loaded with 400 or 500 grains at each end.* The substance intended for analysis is now to be introduced into the tube; if it be solid, as for instance camphor or a like substance, it may be broken into small fragments and shaken down to the bottom; if it be a fluid, as a volatile or fixed oil, it may be introduced by means of a small funnel, as is shown in fig. 7 (Plate XXVII), which funnel is prepared, on the instant, from a piece of flint glass tube of convenient size and substance by heating it near one of its extremities, and suddenly drawing it out; it is evident the semifluid glass will be thus elongated, and a funnel with nearly a capillary tube and of any required length, may be thus obtained; a very little practice will render this part of the business very easy to be accomplished; the funnel is to be put into the tube, reaching very near its bottom or sealed end, and the fluid matter introduced without soiling the upper part of it; care must also be taken on withdrawing the funnel, that no portion of the fluid is attached to its lower extremity, or otherwise this will happen. The volatile substance, or that which is capable of being rendered so by

* The balance I have been in the habit of using was made for me by Mr. Robinson, and is sensibly affected by 1-400th of a grain when loaded with 1000 grains at each end.

a red heat, being now introduced into the tube, its weight is to be very carefully taken, which, when done, the oxide of copper previously freed from its fine dust by the lawn or cyprus sieve,* and recently heated red hot, is to be poured into the tube while warm, to the length of eight or ten inches, having previously put into the tube as much only of perfectly cold oxide as will absorb the fluid portion of matter, and about a quarter of an inch above it, or to stand about the same height above the solid substance. Why I recommend the present proceeding is, that a small quantity of the cold oxide only is used to prevent the hot oxide from coming in contact with the volatile matter which might otherwise endanger the escape of a small portion from the tube, and of course would give erroneous results; and that portion of cold oxide, even if it be fully saturated with moisture, can contain such a very minute quantity of water as not to sensibly affect the accuracy of the analysis. Having proceeded thus far, a quantity of recently ignited asbestos, or spun glass (the former is best), is put into the tube, so as to occupy an inch or two, depending on the quantity of water that is expected to be formed; this must not be crammed, but put rather lightly into the tube. The tube is now to be bent as represented in fig. 1, and its weight may be again taken, but this is not absolutely requisite; it is, however, well to do it. The tube is then to be covered with thin sheet copper, and placed between the forceps, as represented in the same figure, with its open extremity inserted under a jar in the ordinary mercurial pneumatic trough, or it may be connected with a gasometer of Mr. Pepys's construction, which, when ten or twenty grains of a substance are employed, and the quantity of either carbon or azote it contains is considerable, is convenient. Small mercurial graduated jars may be used, even if very large quantities of gas are obtained, as the process of decomposition may at any time be stopped almost instantaneously,† and the quantity contained in them being registered, they may be alternately filled with mercury and displaced by the gaseous products, as long as any comes over, reserving only the last portions for examination, of which a few cubic inches alone are requisite.

The lamps being trimmed with very short wicks are now to be lighted, lighting those first that are nearest the gasometer, and when the tube is red hot, the remaining ones may be set fire to in succession, until the whole length of tube that is filled with the oxide is red hot. One set of lamps for a tube, of the size I have mentioned above, is generally sufficient, but should tubes be used of larger size, such as half an inch in diameter, both sets will then be required. In coating the tube with sheet copper care must be taken not to cover that part of it which contains the asbestos, otherwise the heat will be conducted by it to that portion of tube, and prevent the condensation of the vapour of water, which is very essential; and in placing the tube between the forceps, it will be convenient to allow that part of it which contains the volatile matter to project beyond the forceps; the heat that is conducted by the copper coating is generally enough to volatilize most substances. In the analysis of substances containing much hydrogen, and especially when ten or twelve grains of them are taken, it will be found convenient to attach to the tube a small bulb to contain the water that is generated: this is represented by fig. 6. I believe I have stated the whole that is necessary as respects the management and use of this apparatus as far as regards the decomposition of volatile substances; in the next place, I shall speak of its application to the decomposition of fixed substances, which after what has been said will require but very few words.

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* The finer portion is taken from the oxide to allow more freedom of passage for the vapour through it; in some cases the rush of gas is so sudden, was it not for this precaution, it would be likely to burst the tube.

† I consider this as one of the advantages of this apparatus.

If the substance be a vegetable salt, it must be freed from all extraneous moisture; this is best effected by suffering it to remain over an hygrometric substance in vacuo for some time.

Those who have not the convenience of an air-pump, may content themselves by operating in this way, which, although not quite so elegant, answers the purpose extremely well. A wide-mouthed phial provided with an accurately-ground stopper being procured, select another and much smaller phial that will easily go into it, and allow the stopper of the larger one to close accurately; it is as well to apply a little tallow to the stopper to ensure its more perfect fitting; strew on the bottom of the larger phial a quantity of chloride of calcium (dry muriate of lime), put into the smaller phial the substance in fine powder intended to be dried, and place this in the larger phial standing on the chloride; moisten a small piece of bibulous paper with alcohol, and put it into the larger phial, but not inside of the smaller one; when thus arranged set fire to the moistened paper, and when it has burned a second or two put the stopper in its place; a very good vacuum is by this means formed, and the process of dessication goes on rapidly. I have repeatedly used this method, and found it succeed very well; I think equally so with that usually adopted by means of the air-pump; although by some it may be ridiculed in these days of elegance and refinement.

The substance in this state is to be mixed with a portion of the oxide recently ignited, but in this case suffered to cool, then as quickly as possible introduced into the tube. As much of the oxide may be used as would occupy an extent of tube equal in proportion to that shown in fig. 5; a quantity of oxide is then to be put upon the mixture, and over this it is sometimes well to put a small quantity of copper filings or scrapings; upon these the asbestos is to be used as above, and the operation of ignition is to be conducted in a somewhat different manner to that last-mentioned.

The lamps in this case are to be lighted at the extremity next the gasometer, and as soon as the gas ceases to be liberated, the next in succession may be employed, and so on to the end; but instead of suffering the whole of them to continue in flame, it is as well to extinguish a portion, and to suffer only about three or four to remain in operation at once, but taking care to ignite the whole extent of tube at the close of the process. The gaseous products being collected, and their bulk noticed, their analysis is to be conducted in the usual manner, taking care, however, in all instances, to observe the precise temperature of the gases, that their bulk, as also the quantity of aqueous vapour they contain, may be estimated,* and either to equalise the internal and external surfaces of the mercury, or to calculate the volume of gas by the difference of mercurial levels.

Description of the Plate.

Fig. 1, *a a* and *b b*, two long spirit lamps, each having ten burners and wicks, the burners of each lamp sloping towards those of the other, as seen in the end view, fig. 2; they are placed in a tin tray, *c c*, mounted on four feet; this tray is perforated in the middle the whole length of the lamps, and as wide as *e e*, fig. 2; the object in making the burners sloping is, that they may clear the lamps and approach each other as near as requisite, and yet leave a clear current of air to the flames, and the tray is perforated and mounted on feet to admit this current.

d d, are springing wires placed at each end of the tray to receive the tube *f f*, which contains the substance to be analyzed, and to hold it over ~~or between the two rows of flames~~; by pressing the finger and thumb on the two shoulders, *g g*, fig. 2, the wires open to receive the tube, and close on letting go; and should the tube be shorter than the lamps, an additional support on a leaden foot, fig. 3, is placed through the opening *e e* of the tray to rise between the flames, and hold the end of the tube; the tubes are hermetically sealed at one end, and the materials then put in while the tube is straight; it is then bent at the other end to suit the mercurial trough.

The tubes are coated with copper foil, wrapped spirally round them; if each succeeding fold lie on half the other there will be a double coat of copper all the way; if it lie on two-thirds, there will be three layers of copper, and so on; by which the glass tube is supported from bending when hot, and becomes very uniformly heated. The spirals are continued beyond the end of the tube, to reach the support, and leave the end within the flames. The dotted lines at *h*, fig. 4, show the end of the tube, short of the support; the foil is secured at the last coil by binding wire, as at *i*.

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* For which very convenient formulæ will be found in the ninth edition of Dr. Henry's Elements of Chemistry.

Fig. 5 shows the foil in act of being wrapped on, also the proportion of the space occupied by the materials; first, the mixture of oxide of copper with the material to be analysed, next pure oxide of copper, or copper filings; and lastly, asbestos. When the quantity of water formed is considerable, the tube is either blown into a bulb, as at *k*, fig. 6, or melted on to one ready prepared, as at *l*.

Fig. 7 is a long funnel, made by drawing out the end of a tube of a suitable thickness at *m*, till it is long and small enough through *n n* to reach to the bottom of the tube, and then cutting it off at *m*, by which liquids may be introduced to the bottom of the tube without wetting the sides.

As the wicks nearest the trough are to be lit first, and the remainder in succession, as the former finish their action, there are upright supports of tin, *o o*, fixed on the lamps, one for each space between the burners, against which to rest a slip of tin *p p*, to prevent the lighted wicks from kindling those next, and it also enables the experimenter to blow out those that have done their duty. In fig. 2, the tin slip, *p p*, is shown by dotted lines, reaching from lamp to lamp: little flat caps are put on each burner when done with, to prevent the waste of spirit. Fig. 8 shows one of these caps, *q*, on its place; *r r*, fig. 1, a shelf fixed to the mercurial trough, to hold the lamps; *s s* the graduated jar. Tin pipes with corks, *w w*, as shown in fig. 2, are the apertures to pour the spirit into the lamps; their places only are marked at *w w*, fig. 1.

ARTICLE IV.

On the Ancient Tin Trade, as described by the Rev. J. Hodgson.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Huel Vor, Cornwall, Jan.* 1, 1824.

ONE of the adventurers in our mine very obligingly lends me the *Annals of Philosophy*, with which I am much amused in the winter evenings. In No. 36, for December, 1823, there is a paper "On the Era when Brass was used in Purposes to which Iron is now applied," by the Rev. J. Hodgson; and I find it mentioned there, "that it is probable that the Egyptians or Phœnicians had made mercantile voyages to this country" (the land of the Britons) "more than 16 centuries before that time" (i. e. the time of Julius Cæsar). "That it was known to the Phœnicians in the time of Homer, his accounts of amber and tin are unquestionable evidence." "And there can be no doubt but that the Greeks and Romans frequented it commonly ever after the destruction of Carthage, if not sooner."

Now, Mr. Editor, this account of the tin trade induced me to make some inquiries of the vicar of my parish, who gave me permission to look at the books in his library; so after my day's work in the mine (of which, Mr. Editor, I am a captain), I had a peep into Dr. Borlase's Natural History, and a few other works, but in none of them can I find that the Egyptians or Phœnicians were such great navigators at the period above mentioned, i. e. about 1700 years before Christ.

In the 31st chapter of Numbers, verse 22, we are told that the Israelites in their wars against the Midianites are directed to keep for their own use the gold, the silver, the brass, the iron, the *tin*, and the lead. If this country were then known, we should feel much obliged to Mr. Hodgson, if he would tell us his authority. Had this country been the only one in which tin was ever found, it would certainly have been a strong ground for presuming that the Phœnicians had traded here at the time of the Trojan war. Tin was generally known in Spain,* and amber† has been found in other places besides the Baltic. Therefore I do not think that there is "unquestionable evidence" that it was known to the Phœnicians in the time of Homer, about eight centuries before Christ. Carthage, we know, was founded by a colony from the Phœnicians nearly 900 years before Christ; and other colonies were planted by them at Tangier, Malaga, Gades, &c. Now, Sir, as I know very few of the old books, I should be glad to be informed where I shall find any account of people before that period venturing into the Atlantic. Where we are left in darkness we may be allowed to reason from analogy. When the Portuguese became acquainted with the use of the compass about three centuries ago, what sort of voyages, and what discoveries, did they make, on the coast of Africa, and how easily were they deterred from prosecuting them by a storm. Cape Non was for some time the extreme point to which they ventured. Will it then be believed that the Phœnicians or Egyptians were in the habit of trading to this country without the aid of the needle 16½ centuries before Christ. It may be in the recollection of those who have read Robertson's America, that a Portuguese in sailing for the Cape of Good Hope was driven by the currents and winds out of his course, and unexpectedly discovered a part of South America. In the same way do I believe that this country was first found; for in those days what navigator, however adventurous, would have quitted the coast of Spain to explore unknown seas? If the trade in tin by sea had been known for so many centuries, how does it happen that Herodotus‡ should have been perfectly ignorant of the matter, and in his mention of tin points to the Eridanus, which he says has a Greek derivation. How comes it that we have no accounts recorded of any voyages made to a great distance beyond the Straits of Gades much earlier than 600 years before Christ. Then with respect to the Greeks trading here, Camden says, that they came here 160 years before Cæsar, and Bochart fixes the period more than a century lower, 117 years before Christ. These Greeks were adventurers who had quitted Samos with the intention of forming a colony on the coast of Egypt, and were driven by a storm through the Straits, near which place they settled. These were the only people of that nation who traded with us. Nor after the destruction of Carthage do I believe that the Romans frequented this country, for if they had done it, the spring tides would certainly have been well known to Cæsar.*

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* Pliny, lib. 34, c. 16.

† Pliny, lib. 37, c. 3.

‡ Thalia, Sect. 115.

I am not unacquainted with the account in Strabo, lib. 3, of the Phœnician vessel being run on shore by the crew when pursued by a Roman. That could not possibly have taken place until the conclusion of the first Punic war, and by that time, and long before the trade in tin had been established across France by the Marseillois, a Greek colony, who had quitted Phocæa, before Christ 539, and had carried on the trade in tin and amber with the Romans, to which Herodotus points, and which is subsequently detailed by Diodorus Siculus.

I trust, Sir, that you will allow these few observations to be inserted in the *Annals*, solely with the view of drawing Mr. Hodgson's attention to the subject, and with the hope that he will kindly favour us with a paper "On the Tin Trade," in one of your future numbers.

I am, Sir, your obedient servant,

A TINNER.

ARTICLE V.

On Fossil Shells. By Lewis Weston Dillwyn, Esq. FRS.† In a Letter addressed to Sir Humphry Davy, Bart. Pres. RS.

As fossil shells are more numerous, and generally occur in a better state of preservation than any other of the organic remains, they have become one of the most interesting objects for geological research, and there is such an exact conformity in the structure of many of these fossils with the living genera, as to render it in the highest degree probable, that the habits of their animals were also similar. By availing ourselves of these analogies, some circumstances attending the distribution of fossil shells may be observed which have hitherto escaped notice, and if you should find them to be sufficiently interesting, or likely to open a new door for inquiry, I beg that you will submit to the Royal Society the following observations on the fossil remains of the Mollusca.

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* Lib. iv. sect. 9.

† From the Philosophical Transactions for 1823, Part II.

Pliny, in describing the shell fish which was supposed to yield the Tyrian die, has observed, 'lingua purpuræ longitudine digitali, qua pascitur perforando reliqua conchylia;' and Lamarck says, that all those molluscæ whose shells have a notch or canal at the base of their apertures, are furnished with similar powers, by means of a retractile proboscis; and in his arrangement of invertebral animals they form a section of the Trachelipodes, with the name of 'Zoophages.' Whether all these Trachelipodes are possessed of the same predaceous powers of boring into hard substances, and whether some of them may not subsist chiefly on dead animals, my own observations have led me greatly to doubt; but this notch or canal is made for the protrusion of a trunk, which is formed to answer the same purposes as the respiratory organs of a Gastrobranchus,* and may serve at once to distinguish a carnivorous species. The following fossil genera belong to this section of the Trachelipodes—Conus, Oliva, Ancilla, Terebellum, Seraphs, Cypræa, Ovula, Volvaria, Marginella, Voluta, Mitra, Terebra, Buccinum, Harpa, Monoceros, Purpura, Cassis, Cassidaria, Strombus, Rostellaria, Triton, Murex, Ranella, Pyrula, Fusus, Cancellaria, Potamides, and Cerithium.

In all the other genera of turbinated univalves, the lower margin of the aperture, instead of being either notched or channelled, is entire; and Adanson, in his History of Senegal, so far back as 1757, has shown that the Molluscæ of these shells have jaws which are formed for feeding on vegetable substances; and they have been proved, by subsequent observations, to be entirely herbivorous, i. e. the marine genera feed on algæ, and the fresh water and land genera on the leaves of vegetables. These together constitute the other section of the Trachelipodes, which Lamarck has called 'Phytiphages,' and it comprises the following genera of fossils—Turritella, Turbo, Cirrus, Euomphalus, Trochus, Solarium, Delphinula, Scalaria, Natica, Nerita, Ampullaria, Vivipara,† Paludina, Melania, Planorbis, Cyclostoma, Auricula, Tornatella, Bulimus, Helicina, and Helix.

Every turbinated univalve of the older beds from transition lime to the lias, which I have been able to procure, or of which I can find any record, belongs to these herbivorous genera, and the family has been handed down through all the successive strata, and still inhabit our land and waters. On the other hand, all the carnivorous genera abound in the strata above the chalk, but are comparatively extremely rare in the secondary strata, and not a single shell has been detected in any older bed than the lower oolite. As a proof of this rarity it may be remarked, in the list of British fossils which Mr. Parkinson has given in his Introduction to the Study of Organic Remains, that not one single species of either of the carnivorous genera has been referred to any stratum below the London clay, and only the few following species appear in any of the numerous lists of the secondary strata which are given in Conybeare and Phillips's Outlines of Geology, viz. a Murex* and *Pleurotoma rostrata* in the green sand, *Cerithium melanoides* in chalk marle, and a few species of Rostellaria in various strata from chalk marle to the lower oolite. For the Pleurotoma and the Cerithium, a reference to the Mineral Conchology is given; and Mr. Sowerby there only says that he has seen an imperfect cast, very like the former, from the canal at Devizes; and of the latter, that it was found in the London clay, and in the clay *above* the chalk at Newhaven. It is also worthy of remark, that all the above-mentioned Rostellariæ which have been found in secondary strata are nearly allied to the Linnæan *Strombus Pes Pelecani*; and it may be observed that this species, when fully grown, has not any open canal at its base; and that in the figure which Muller has given of the animal there is no appearance, nor in Montagu's description is any mention made, of that retractile proboscis or respiratory trunk, which are the distinguishing characters of a carnivorous Trachelipode. I therefore propose to remove these Rostellariæ of the secondary strata, which are readily distinguished by the remarkable expansion of their outer lips, to form a separate genus with Petiver's name of Aporrhais and the other fossil Rostellariæ which have the recent *Strombus fusus*, for their type are only to be found in strata above the chalk.

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* See Sir E. Home's observations on this animal under the name of Myxine, in the Philosophical Transactions for 1815, p. 261.

† I am unable to distinguish this genus from Paludina; and the name of Vivipara is calculated to mislead, for none of the species are more than ovi-viviparous.

Small circular holes, which have been bored by the predaceous Trachelipodes, are frequently found in recent shells, and I have seen exactly similar holes in many fossils, but they have all been taken from the London clay or crag; nor have I been able to find any such appearance in any fossil of the older formations. If this observation should be confirmed by a more extended examination of other cabinets, it will prove that neither the Aporrhaides, or any of those few undoubtedly carnivorous species which have been found in the secondary formations, were furnished with any such predaceous powers as Pliny has described, and that they belong to a subdivision of the Trachelipoda zoophaga, which feed only on dead animals. Without attempting to distinguish the more predaceous from these other genera, I shall however at present content myself with proving, and for this I have adduced sufficient evidence, that the whole family of the carnivorous Trachelipodes are extremely rare in all those strata where the Ammonites and other Nautilidæ abound.

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* Mr. George Sowerby has sent me this shell with the name of *Murex calcar*, and if I am not much mistaken, I have seen another species of Murex from the green sand in the extensive collection of Mr. J. S. Miller.

In describing the Ammonites, De Montfort, in his Conchologie Systematique, observes, that they are found of all sizes, "depuis la grandeur d'une Lentille jusqu'a celle de 8 pieds de diametre;" and, as a proof of their great abundance, Lamarck says, "La route d'Auxerre à Avalon, en Bourgogne, est ferrée avec des Cornes d'Ammon." These Ammonites, as well as most of the other principal multilocular genera, appear to have become extinct in our northern latitudes when the chalk formation was completed; but a few of the Nautilidæ still inhabit the southern ocean, and their molluscæ belong to the carnivorous order which Lamarck has described under the name of Cephalopodes. From the occurrence in such great numbers of the carnivorous Trachelipodes in the formation above the chalk, it therefore appears, that the vast and sudden decrease of one predaceous tribe has been provided for by the new creation of many genera, and a myriad of species possessed of similar appetencies, and yet formed for obtaining their prey by habits entirely different from those of the Cephalopodes.

It may be further observed, that all the marine genera of the herbivorous Trachelipodes to which either of the fossil species belongs, are furnished with an operculum, and that the few carnivorous species which have been found in the secondary strata, agree with them in this particular, although the unoperculated genera are very abundant in the London clay. Lamarck, of the fresh water Trachelipodes says, that those which are not furnished with an operculum are formed for the occasional respiration of air; but I believe that this observation is not applicable to the marine genera; and it was Adanson's opinion, that the operculum is intended for the protection of the animal; nor can I imagine any thing against which such a shield would be more necessary than the long and pliable fingers of the Cephalopodes, when they abounded in the seas, as they must formerly have done. It is, therefore, at least a curious coincidence, that all the marine Trachelipodes of the transition and secondary strata, of which I can find any record, belong to genera which are furnished with an operculum, and that none of the numerous unoperculated genera should have been found in any other than the tertiary formations where the Ammonites disappear. For the protection of the testaceous Gasteropodes no such shield would be wanting, and including this order it may be generally observed, that none of the marine unoperculated Mollusca, except the Cephalopodes, are to be found in the lias, or in any of its older strata; and it appears to me that a much greater approach towards the same variety of testaceous animals which now inhabits our seas is to be found in the adjoining bed of lower oolite.

The foregoing observations are confined chiefly to British fossils; for as a few of the testaceous Cephalopodes still live in the warmer climates, it is possible that the Ammonites, as well as some others of the extinct genera, may have existed longer, and that their remains may be found in the tertiary formations of the more southern latitudes. Although fossil Nautilidæ are common in the secondary strata of the United States, they are said not to have been found in South America; and it may, therefore, be queried whether the Cephalopodes were not confined to the more northern latitudes when the chalk formation was completed, and whether a decrease in the earth's temperature at that period may not have occasioned the entire destruction of some genera, and a migration of others to the southward.

It is highly probable, when a more perfect knowledge of the testaceous animals has been obtained, that the line of inquiry which I have now suggested may be greatly extended, and the collected tendency of such analogies between the habits of living animals and the organic remains of the different strata, may serve to throw some light on the nature of the changes which the surface of our planet has undergone.

Article VI.

On the active Power of Dilatation of the Heart.[*]

By David Williams, MD.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Liverpool, Feb. 7, 1824.*

The following observations were made during my experiments on the practicability of an operation for phthisis pulmonalis,[†] and also while inquiring into the cause and the effects of an obstruction of the blood in the lungs.[‡] By securing the trachea of an animal at the acme of inspiration, the heart continues its action for some time. To ascertain the strength of the active power of dilatation attributed to the right auricle and ventricle, the cavæ were compressed, the inferior as near the diaphragm as possible, and the superior, a little above its entrance into the auricle. As the auricle was thus suddenly limited to the small quantity of blood, that the vena azygos and the coronary veins poured into it, it was expected that the blood, which remained in the inferior cava between the compressed part and the auricle would have been pumped out; but no effect indicating a sudden extraction of blood from the isolated portion of the inferior cava could be perceived. Had the auricle or ventricle exercised the function of active dilatation, it must have been discovered, for the lungs were quiescent, and no muscular action, save that of the heart itself, perplexed the observation. After the last systole of the left ventricle has occurred, irregular and hurried, or fluttering contractions of the muscular fibres of the right ventricle take place. When they have ceased, the right ventricle feels full and soft, and the left feels contracted or collapsed. If now we open one of the pulmonary veins near their termination in the auricle, no expansion of the latter chamber or of the ventricle will take place; but as the residue of the blood in the pulmonary veins drains into the ventricle, it imperceptibly fills, and its walls feel softer to the touch.

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[*] Extracted from An Essay on the Motive Powers of the Circulation of the Blood, read before the Literary and Philosophical Society of Liverpool, Jan. 1824.

[†] *Annals* for June.

[‡] *Annals* for September.

We shall now take into consideration the influence ascribed to the active power of dilatation of the heart in the economy of the circulation. In the first place, we shall inquire into the nature of the power; then compare its characteristic qualities with the above phenomena; afterwards we shall be able to judge whether we have arrived at any facts capable of furthering our acquaintance with the moving powers of the circulation. As Dr. Wilson Philip is the last author on our subject whose writings I have perused, I shall take the liberty of quoting the following periods from his valuable essay,* so as to give my readers a correct statement of the ideas entertained by Dr. Philip, as to the nature and the influence exercised by the inherent dilating power of the heart, and also the resilience of the lungs. "What purpose then," says Dr. Philip, "is served by the dilating power of the ventricles increased by the tendency of the lungs to collapse? It favours the entrance of the blood suddenly propelled into it by the contraction of the auricle; and the degree of dilating power is well proportioned to this office. Without this dilating power, the tendency of the ventricle would be to remain in a state of collapse after the systole, and part of the power of the auricle would be expended in dilating the ventricle. Here, as in many other instances, both in man and the inferior animals, we see nature saving the muscular by the substitution of the elastic power." In the last sentence we perceive Dr. Philip recognizing elasticity to be the nature of the inherent power which enables the ventricles to dilate themselves. Pray what is the nature of the action of an elastic body? Mr. John Hunter thus defines it: "The action of elasticity is continual, and its immediate effects are produced whenever the resistance is removed; by which it may be distinguished from other powers."* From our definition, we learn that the action of an elastic body is permanent, and that as soon as the resisting power which retains it in a forced position is removed, that it immediately regains its natural state of rest. In order to the elucidation of our problem, we shall admit the body of the left ventricle of the heart to be possessed of an elastic property. As the systole of the ventricle throws the elastic property into a forced position, and as the ventricle remains for some time after its last systole in a comparative state of collapse, we have only to do away with the influence of the power which retains it during that period in that state, and the elastic property will instantly restore itself to its natural position. Before we set about releasing it from its constrained situation, we shall inquire into the nature of the power which we have to contend with. As a state of relaxation in a muscular fibre succeeds the state of contraction, it follows that the action of the muscular fibres of the walls of the ventricle cannot be the cause of the confinement of the elastic property in its unnatural position, for we admit contraction to be the last motion of the ventricle. The resisting power then must arise from the propulsion of a portion of the blood into the aorta from the cavity of the ventricle by its systole, without its being able (by its elasticity or active power of dilatation) to draw its wonted supply in return from the auricle, on account of the latter being itself deprived of its usual supply. Therefore, as the elastic property endeavours by its reaction to regain its natural state of rest, a tendency to form a vacuum in the cavity of the ventricle must be the result, which effectually retains the elastic property in its constrained position. Now if we can establish a communication between the cavity of the ventricle and the exterior air, it is evident that we shall do away with the tendency to a vacuum, and consequently with the resistance offered to the reaction of the elastic property. Such a communication is easily established without doing any injury to the walls of the ventricle, by opening one of the pulmonary veins, near their junction with the auricle. In the narration of our experimental investigation, we are informed that after such an expedient was had recourse to, that no such phenomenon as dilatation of the ventricle was remarked. Therefore if we can depend on the correctness of the observations during the above experiments, and if observations under such circumstances can be relied on, we must conclude the active power of dilatation, or the action of the elastic property of the auricles and ventricles, to be either ideal, or to be so extremely feeble, as to be capable of evading our senses under the most favourable situation in which we can place the organs for inspection. For I can scarcely conceive it possible to devise more decisive modes of ascertaining the existence of such a power, though ever so trifling, than those had recourse to.

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* Some Observations relating to the Powers of Circulation, &c. Medico-Chir. Trans. Vol. xii. Part. II.

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* On the Blood.

Further, as the action of an elastic substance is as perfect after the extinction of life, until the process of putrefaction destroys its texture, as during the existence of the animal; by examining a heart detached, after its absolute death, or after its utmost contraction by the vis mortua, we can readily satisfy ourselves, whether the walls of the ventricles have any elastic property that can be appreciated. If we find a heart contracted, and on pressing its body so as to flatten it, that it does not present to our senses a disposition to recover its natural shape similar to what we witness in the truly elastic arteries whose roots are attached to its base, what inference are we to draw? Why certainly we must need infer that it possesses no greater elastic property than muscles in common. The condition of the heart greatly depends on the state of the animal when killed. Fat beasts (more particularly sheep), from their unwieldiness, and from the action of their diaphragms being restrained by their obesity, are easily overdriven, and sometimes on their way to the slaughter-house, to prevent their suffocating, they are obliged to be "*stuck;*" or from urgency, they are killed while yet breathless and ready to faint. The right ventricle of the heart of an animal killed in such a plight is found to be gorged, and the reason appears to me to be obvious. As the blood is more or less obstructed in its passage through the lungs, previous to the sticking of the animal, the pulmonary artery and the cavities of the right side of the heart are necessarily more or less gorged, and the ventricle and pulmonary artery must remain so; for during the time the animal is bleeding to death, a small portion only of the blood which they contain at the time the animal is stabbed, can pass into the system of the pulmonary veins, for want of pure air in the air cells of the lungs to enable it to undergo the mysterious change in the rete Malpighii. Thus the right ventricle will be large and flabby, or with its muscular fibres relaxed after the pluck is extracted; for in consequence of its being retained in a state of extension, the action of the vis mortua is prevented from affecting its muscular fibres. When an animal is killed by dividing the blood-vessels of the neck without any previous obstruction in the lungs, then no engorgement can take place in the right ventricle, for the blood rushes with unusual impetuosity towards the point where there is the least resistance, and in a few minutes nearly the whole of the blood in the body escapes through the artificial outlets. I think the gorged state of the right ventricle of the heart of an animal when killed, while breathless and faint, to be another evidence in support of my opinion, that prostration of strength, arising from short continuance of anxious exertion, is the immediate effect of an obstruction of blood in the lungs.*

ARTICLE VII.

A Table of Equivalent Numbers.

[SINCE the publication of any table of equivalents in the *Annals*, various important additions have been made to this department of chemistry; new editions of Dr. Thomson's and of Dr. Henry's treatises on chemistry have appeared, and Mr. Brande has published a table in the Institution Journal. In the present table, I have inserted corrections obtained from various sources, and a few as the results of my own experiments.—*Edit.*]

TABLE I.

ACID, acetic	50	Acid, iodic	165
crystallized (1 water)	59	malic	70
arsenic	62	molybdic	72
arsenious	54	molybdous	64
benzoic	120	muriatic	37
boracic ?	22	nitric (dry)	54
crystallized (2 water)	40	liquid (sp. gr. 1·50 2 water)	72
carbonic	22	nitrous	46
chloric	76	oxalic	36
chloriodic	161	crystallized (4 water)	72
chlorocarbonic	50	perchloric	92
chlorocyanic	62	phosphoric	28
chromic	52	phosphorous	20
citric	58	saclactic	105
crystallized (2 water)	76	succinic (anhydrous crystals)	50
columbic ?	152	sulphuric (dry)	40
ferrocyanic	?	liquid (sp. gr. 1·4838)	49
fluoboric ?	22	sulphurous	32
fluoric	17	tartaric	67
formic	37	crystallized (1 water)	76
fluosilicic	24	tungstic	120
gallic ?	63	uric	45 ?
hydriodic	126	Alum (dry)	262
hydrocyanic	27	(crystallized 25 water)	487
hydrofluoric	17	Alumina	27
hyposulphurous	24	sulphate	67
hyposulphuric	36	subsulphate (2 acid, 3 base)	116

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* Medical and Surgical Journal for Oct. 1823, p. 535.

Aluminum	10
Ammonia	17
acetate	67
bicarbonate (2 water)	79
borate ? (dry)	39
crystallized (2 water)	57
carbonate	39
sesquicarbonate (2 water)	118
citrate	75
fluoborate	39
hydriodate	143
iodate	182
molybdate	89
muriate	54
nitrate	71
oxalate	53
(cryst. 1 water)	62
phosphate	45
phosphite	37
succinate	67
sulphate	57
sulphite	49
tartrate	84
potassa-tartrate	208
Antimony	44
chloride	80
iodide	169
deutoxide	56
peroxide	60
protoxide	52
sulphuret	60
potassa-tartrate	?
Arseniate of ammonia	79
potash	110
soda	94
Arsenic	38
acid	62
chloride	?
iodide	?
Arsenious acid	54
Azote	14
Barium	70
chloride	106
iodide	195
peroxide	86
phosphuret	82
sulphuret	86
Barytes	78
acetate	128
arseniate	140
arsenite	132
benzoate	198
borate	100
carbonate	100
chlorate	154
chromate	130
citrate	136
hydrate	87
iodate	243
nitrate	132
muriate (cryst. 1 water)	124
oxalate	114
phosphate	106
phosphite	98
succinate	128
sulphate	118
sulphite	110
tartrate	145
tungstate	198
Benzoic acid	120
Bicarburetted hydrogen	7
Bismuth	72
acetate	130
arseniate	142
benzoate	200
chloride	108
citrate	138
iodate	245
iodide	197
nitrate	134
oxalate	116
oxide	80
phosphate	107
phosphuret	84
sulphate	120
sulphuret	88
tartrate	147
Boracic acid	22 ?
acid crystallized (2 water)	40
Borax (8 water)	158
Boron	6 ?
Cadmium	56
carbonate	86
chloride	92
iodide	181
nitrate	118
oxide	64
phosphate	92
phosphuret	68
sulphate	104
sulphuret	72
Calcium	20

chloride	56
fluoride	36
iodide	145
oxide (lime)	28
phosphuret	32
sulphuret	36
Calomel	236
Camphoric acid	?
Carbon	6
perchloride	120
protochloride	42
subchloride	48
hydrochloride	50
oxide	14
phosphuret	18
sulphuret	38
Carbonic acid	22
oxide	14
Carburet of azote	26
sulphur	38
phosphorus	18
Carburetted hydrogen	8
Cerium	46 ?
Chloric acid	76
Chlorine	36
Chromium	28
deutoxide	44
oxide	36
Cobalt	26
acetate	84
arseniate	96
benzoate	154
borate	56
carbonate	56
chloride	62
citrate	92
iodide	151
nitrate	88
oxalate	70
peroxide	?
phosphate	62
phosphuret	38
protoxide	34
sulphate (dry)	74
crystallized (7 water)	137
sulphuret	42
tartrate	101
Columbium	144 ?
Copper	64
acetate	130
cryst. (6 water. Com verdigris)	184
binacetate	180
cryst. (3 water. dist. verdigris)	207
subacetate (1 acid 2 base)	210
carbonate (unhydrous)	102
(2 water, malachite)	111
iodide	189
perchloride	136
pernitrate	188
persulphate	160
crystallized (10 water)	250
perphosphate	136
phosphuret	76
protochloride	100
protoxide	72
peroxide	80
sulphuret	80
Corrosive sublimate	272
Cyanogen	26
Fluorine	16
Glucina	26
Glucinum	18
Gold	200
chloride	236
iodide	325
protoxide	1 + 1 = 208
peroxide	1 + 3 = 224
sulphuret	1 + 3 = 248
chloride of, & sodium (dry)	296
crystallized, 8 water	368
Hydrogen	1
Iodine	125
Iron	28
protochloride	64
perchloride	82
peroxide	40
protoxide	36
sulphate (dry)	76
crystallized (7 water)	139
persulphuret	60
protosulphuret	44
Lead	104
acetate	162
crystallized (3 water)	189
sub-binacetate	274
sub-tritacetate	386
arseniate	174
benzoate	232
borate	134
carbonate	134
chlorate	188
chloride	140

chromate	164
citrate	170
deutoxide	116
iodate	277
iodide	229
malate	182
molybdate	184
nitrate	166
oxalate	148
peroxide	120
phosphate	140
phosphite	132
phosphuret	116
protoxide	112
succinate	162
sulphate	152
sulphite	144
sulphuret	120
tartrate	179
Lime	28
acetate	78
arseniate	90
benzoate	148
biphosphate	84
borate	50 ?
carbonate	50
chlorate	104
chloride	64
citrate	86
chromate	80
hydrate	37
iodate	193
muriate cryst. (5 water)	110
oxalate	64
phosphate	56
phosphite	48
succinate	78
sulphate	68
crystallized (2 water)	86
sulphite	60
tartrate	95
tungstate	148
Lithia	18
carbonate	40
nitrate	72
phosphate	46
sulphate	58
Lithium	10
chloride	46
iodide	135
sulphuret	26
Magnesia	20
ammonia-phosphate	93
borate ?	42
carbonate	42
hydrate	29
muriate	57
nitrate	74
phosphate	48
sulphate (dry)	60
crystallized (7 water)	123
tartrate	87
Magnesium	12
chloride	48
iodide	137
phosphuret	24
sulphuret	28
Manganese	28
acetate	86
benzoate	156
carbonate	58
chlorate	112
chloride	64
citrate	94
deutoxide	40
oxalate	72
peroxide	44
phosphate	64
phosphuret	40
protoxide	36
succinate	86
sulphate	76
tartrate	103
Mercury	200
bipersulphate	296
bisulphuret	232
bicyanuret	252
perchloride	272
periodide	450
pernitrate	324
peroxide	216
perphosphate	272
persulphate	256
protochloride	236
protonitrate	262
protosulphate	248
protoxide	208
Molybdenum	48
protoxide	56
Nickel	29
acetate	87
arseniate	99

benzoate	157
borate	59
carbonate	59
chloride	65
citrate	95
iodide	154
nitrate	91
oxalate	73
peroxide	?
phosphate	65
phosphuret	41
protoxide	37
sulphate (dry)	77
crystallized (7 water)	140
sulphuret	45
tartrate	104
Nitric oxide	30
Nitrogen	14
Nitrous oxide	22
Olefiant gas	7
Osmium	?
oxide	?
Oxygen	8
Palladium	?
oxide	?
Phosphorus	12
carburet	18
chloride	48
perchloride	84
sulphuret	28
Platinum	96
ammonia-muriate	196
perchloride	142
peroxide	112
bi-phosphuret	120
bi-sulphuret	128
Potash (dry)	48
acetate	98
arseniate	110
arsenite	102
benzoate	168
bicarbonate	92
crystallized (1 water)	101
bichromate	152
binarseniate	172
binoxalate	120
biphosphate	104
bisulphate	128
crystallized (1 water)	137
bitartrate	182
crystallized (1 water)	191

borate	70?
carbonate	70
chlorate	124
chromate	100
citrate	106
hydrate	57
iodate	213
molybdate	120
nitrate	102
oxalate	84
phosphate	76
quadroxalate.	192
succinate	98
sulphate	88
sulphite	80
tartrate	115
tungstate	168
Potassium	40
chloride	76
iodide	165
peroxide	64
phosphuret	52
protoxide (dry)	48
sulphuret	56
Rhodium	44 ?
peroxide	60
protoxide	52 ?
Selenium ?	41
Silica	16
Silicium	8
Silver	110
acetate	168
arseniate	180
arsenite	172
benzoate	238
borate ?	140
carbonate	140
chlorate	194
chloride	146
chromate	170
citrate	176
iodate	283
iodide	235
molybdate	190
nitrate	172
oxalate	154
oxide	118
phosphate	146
sulphate	158
sulphite	150
sulphuret	126

tartrate	185
tungstate	238
Soda	32
acetate	82
crystallized (6 water)	136
arseniate	94
arsenite	86
benzoate	152
bicarbonate	76
borate ?	54
carbonate (dry)	54
crystallized (11 water)	153
chlorate	108
chromate	84
citrate	90
hydrate	41
iodate	197
molybdate	104
nitrate	86
oxalate	68
succinate	82
sulphate (dry)	72
crystallized (10 water)	162
sulphite	64
tartrate	99
and potash	214
Sodium	24
chloride	60
iodide	149
phosphuret	36
peroxide	36
protoxide	32
sulphuret	40
Starch ?	142
Strontia	52
acetate	102
borate ?	74
carbonate	74
citrate	110
hydrate	61
muriate (cryst. 5 water)	134
oxalate	88
phosphate	80
sulphate	92
tartrate	119
Strontium	44
chloride	80
iodide	169
phosphuret	56
sulphuret	60
Sugar	?
Sulphur	[illegible]
carburet	[illegible]
chloride	[illegible]
iodide	[illegible]
phosphuret	[illegible]
Sulphuretted hydrogen	[illegible]
Tannin ?	[illegible]
Tellurium	38
chloride	74
oxide	46
Tin	58
bisulphuret	90
iodide	183
peroxide	74
protoxide	66
perchloride	130
protochloride	94
sulphuret	74
phosphuret	70
Titanium	?
Tungsten	96
Tungstic acid	120
Uranium	?
oxide	?
Uric acid	45?
Water	9
Yttria	40
Yttrium ?	32
Zinc	34
acetate	92
arseniate	104
benzoate	162
borate	64
carbonate	64
chlorate	118
chloride	70
citrate	100
iodate	207
iodide	159
nitrate	96
oxalate	78
oxide	42
phosphate	70
phosphuret	46
succinate	92
sulphate (dry)	82
crystallized (6 water)	136
sulphite	74
tartrate	110
Zirconia	45?
Zirconium	37 ?

benzoate	157
borate	59
carbonate	59
chloride	65
citrate	95
iodide	154
nitrate	91
oxalate	73
peroxide	?
phosphate	65
phosphuret	41
protoxide	37
sulphate (dry)	77
crystallized (7 water)	140
sulphuret	45
tartrate	104
Nitric oxide	30
Nitrogen	14
Nitrous oxide	22
Olefiant gas	7
Osmium	?
oxide	?
Oxygen	8
Palladium	?
oxide	?
Phosphorus	12
carburet	18
chloride	48
perchloride	84
sulphuret	28
Platinum	96
ammonia-muriate	196
perchloride	142
peroxide	112
bi-phosphuret	120
bi-sulphuret	128
Potash (dry)	48
acetate	98
arseniate	110
arsenite	102
benzoate	168
bicarbonate	92
crystallized (1 water)	101
bichromate	152
binarseniate	172
binoxalate	120
biphosphate	104
bisulphate	128
crystallized (1 water)	137
bitartrate	182
crystallized (1 water)	191

borate	70?
carbonate	70
chlorate	124
chromate	100
citrate	106
hydrate	57
iodate	213
molybdate	120
nitrate	102
oxalate	84
phosphate	76
quadroxalate.	192
succinate	98
sulphate	88
sulphite	80
tartrate	115
tungstate	168
Potassium	40
chloride	76
iodide	165
peroxide	64
phosphuret	52
protoxide (dry)	48
sulphuret	56
Rhodium	44?
peroxide	60
protoxide	52?
Selenium?	41
Silica	16
Silicium	8
Silver	110
acetate	168
arseniate	180
arsenite	172
benzoate	238
borate?	140
carbonate	140
chlorate	194
chloride	146
chromate	170
citrate	176
iodate	283
iodide	235
molybdate	190
nitrate	172
oxalate	154
oxide	118
phosphate	146
sulphate	158
sulphite	150
sulphuret	126

Boracic acid (crystallized	40
Sulphuric acid	
Potassium	
Yttria ?	
Sulphuret of sodium	
Carbonate of lithia	
Deutoxide of manganese	
Peroxide of iron	
Phosphuret of manganese	
Phosphuret of iron	
5 Oxygen	
Hydrate of soda	41
Phosphuret of nickel	
Selenium ?	
Protochloride of carbon	42
Oxide of zinc	
Carbonate of magnesia	
Borate of magnesia ?	
Sulphuret of cobalt	
Protoxide of chlorine	44
Strontium	
Peroxide of manganese	
Deutoxide of Chromium	
Rhodium ?	
Protosulphuret of iron	
Antimony	
Phosphate of ammonia	45
Zirconia ?	
Sulphuret of nickel	
Uric acid ?	
5 Water	
Nitrous acid	46
Chloride of lithium	
Phosphate of lithia	
Phosphuret of zinc	
Oxide of tellurium	
Cerium ?	
Protochloride of phosphorus	48
Potash (dry)	
Phosphate of magnesia	
Subchloride of carbon	
Molybdenum ?	
Chloride of magnesium	
Phosphite of lime ?	
6 Oxygen	
Sulphite of ammonia	49
Liquid sulphuric acid (1 water)	
Hydrochloride of carbon	50
Carbonate of lime	
Borate of lime	
Acetic acid	
Sulphuret of zinc.	
Succinic acid ?	
Chlorocarbonic acid	
Chloride of sulphur	52
Protoxide of rhodium	
Phosphuret of potassium	
Protoxide of antimony	
Strontia	
Chromic acid	
Oxalate of ammonia	53
Dry nitric acid	54
Muriate of ammonia	
Carbonate of soda	
Protoxide of cerium	
Arsenious acid	
6 Water	
Sulphuret of potassium	56
Deutoxide of antimony	
Chloride of calcium	
Phosphate of lime	
Phosphuret of strontium	
Protoxide of molybdenum	
Carbonate of cobalt	
Borate of cobalt ?	
Cadmium	
7 Oxygen	
Sulphate of ammonia	57
Hydrate of potash	
Borate of ammonia (2 water)	
Muriate of magnesia	
Tin	
Cabonate of manganese	
Sulphate of lithia	58
Citric acid (dry)	
Borate of nickel ?	59
Acetic acid crystallized	
Carbonate of nickel	
Chloride of sodium	60
Persulphuret of iron	
Peroxide of rhodium	
Phosphate of soda	
Sulphate of lime	
Sulphate of magnesia (dry)	
Sulphuret of antimony	
Peroxide of antimony	

Hydrate of strontia	61
Phosphate of cobalt	62
Chloride of cobalt	
Arsenic acid	
Chlorocyanic acid	
Oxalate of ammonia (1 water)	
Gallic acid	63
7 Water	
Peroxide of potassium	64
Sulphite of soda	
Chloride of manganese	
Protochloride of iron	
Oxide of cadmium	
Copper	
Molybdous acid	
Sulphuret of molybdenum	
Phosphate of manganese	
Chloride of lime	
8 Oxygen	
Carbonate of zinc	
Borate of zinc ?	
Oxalate of lime	
Chloride of nickel	65
Phosphate of nickel	
Protoxide of tin	66
Sulphate of alumina	
Sulphate of alumina	67
Tartaric acid (dry)	
Acetate of ammonia	
Succinate of ammonia	
Ferrocyanic acid	
Peroxide of chlorine	68
Sulphate of lime	
Phosphuret of cadmium ?	
Oxalate of soda	
Carbonate of potash	70
Borate of potash ?	
Oxalate of cobalt	
Chloride of zinc	
Phosphate of zinc	
Barium	
Acetate of magnesia	
Phosphuret of tin	
Malic acid	
Nitrate of ammonia	71
Tannin ?	
Liquid nitric acid (2 water)	72
Crystallized oxalic acid (4 water)	
8 Water	
Sulphate of soda (dry)	
Nitrate of lithia	
Protoxide of copper	
Oxalate of manganese	
Bismuth	
Molybdic acid	
Sulphuret of cadmium	
Carbonate of strontia	
Borate of strontia ?	
9 Oxygen	
Oxalate of nickel	73
Nitrate of magnesia	74
Sulphuret of tin	
Peroxide of tin	
Sulphite of zinc	
Sulphate of cobalt (dry)	
Chloride of tellurium	
Citrate of ammonia	75
Chloric acid	76
Crystallized citric acid (2 water)	
Crystallized tartaric acid (1 water)	
Chloride of potassium	
Phosphate of potash	
Phosphuret of copper	
Bi-carbonate of soda (dry)	
Protosulphate of manganese	
———— iron (dry)	
Sulphate of nickel (dry)	77
Baryta	78
Acetate of lime	
Succinate of lime	
Arseniate of ammonia	79
Bi-carbonate of ammonia (2 water)	
Oxalate of zinc	
Sulphite of potash	80
Oxide of bismuth	
10 Oxygen	
Peroxide of copper	
Protosulphuret of copper	
Chromate of lime	
Phosphate of strontia	
Chloride of strontium	

Chloride of antimony	81
Sugar ?	
Nitrate of lime	82
Phosphuret of barium	
Perchloride of iron	
Sulphate of zinc (dry)	
Acetate of soda	
Succinate of soda	
Bi-chloride of phosphorus	84
Phosphuret of bismuth	
Bi-phosphate of lime	
Chromate of soda	
Acetate of cobalt	
Oxalate of potash	
Tartrate of ammonia	
Arsenite of soda	86
Sulphate of lime cryst. (2 water)	
Carbonate of cadmium	
Acetate of manganese	
Succinate of manganese	
Peroxide of barium	
Sulphuret of barium	
Nitrate of soda	
Citrate of lime	
Acetate of iron	
Hydrated baryta	87
Acetate of nickel	
Tartrate of magnesia	
Sulphate of potash	88
Sulphuret of bismuth	
Nitrate of cobalt	
Oxalate of strontia	
11 Oxygen	
Molybdate of ammonia	89
Arseniate of lime	90
10 Water	
Nitrate of manganese	
Bi-sulphuret of tin	
Protonitrate of iron	
Citrate of soda	
Nitrate of nickel	91
Perchloric acid	92
Bi-carbonate of potash (dry)	
Chloride of cadmium	
Citrate of cobalt	
Phosphate of cadmium	
Sulphate of strontia	
Acetate of zinc	
Succinate of zinc	
Chlorate of ammonia	93
Ammonia-phosphate of magnesia	
Citrate of manganese	94
Protochloride of tin	
Arseniate of soda	
Tartrate of lime	95
Citrate of nickel	
Arseniate of nickel	96
Platinum	
Nitrate of zinc	
Tungsten ?	
12 Oxygen	
Succinate of potash	98
Acetate of potash	
Phosphite of baryta	
Tartrate of soda	99
Arseniate of nickel	
11 Water	
Carbonate of barytes	100
Borate of barytes ?	
Persulphate of iron	
Citrate of zinc	
Protochloride of copper	
Chromate of potash	
Bi-carbonate of potash cryst. (1 w.)	101
Tartrate of cobalt	102
Nitrate of potash	
Acetate of strontia	
Arsenite of potash	
Carbonate of copper	
Prototartrate of manganese	103
——— iron	
Bi-phosphate of potash	104
Chlorate of lime	
Arseniate of zinc	
Molybdate of soda	
Tartrate of nickel	
Sulphate of cadmium	
Lead	
Ammonia-phosphate of soda	105
Saclactic acid ?	
Chloride of barium	106
Phosphate of barytes	
Citrate of potash	

Chlorate of soda	108
Phosphate of bismuth	
Chloride of bismuth	
12 Water	
Cryst. nitrate of lime (3 water)	109
Nitrate of strontia.	
Silver	110
Muriate of lime cryst. (5 water)	
Arseniate of potash	
Tartrate of zinc	
Citrate of strontia	
Sulphite of barytes	
Protoxide of lead.	112
Chlorate of manganese	
Oxalate of barytes	114
Tartrate of potash	115
Phosphuret of lead	116
Oxalate of bismuth	
Deutoxide of lead	
Crystallized carbonate of soda (7 water).	117
13 Water.	
Hydrated carbonate of ammonia	118
Sulphate of barytes	
Nitrate of cadmium	
Chlorate of zinc	
Oxide of silver	
Tartrate of strontia	119
Peroxide of lead.	120
Sulphate of bismuth	
Sulphuret of lead.	
Tungstic acid	
Molybdate of potash	
Binoxalate of potash	
Benzoic acid	
Perchloride of carbon	
Rhodium ?	
Crystallized sulphate of magnesia (7 water)	123
Chlorate of potash	124
Muriate of barytes (2 water)	
Iodine	125
Hydriodic acid	126
Sulphuret of silver	
14 Water.	
Bi-sulphate of potash	128
Protoxide of rhodium ?	
Succinate of barytes	
Acetate of barytes	
Acetate of bismuth	130
——— of copper.	
Chromate of barytes	
Perchloride of tin	
Nitrate of barytes	132
Arsenite of barytes	
Phosphite of lead.	
Nitrate of bismuth	134
Carbonate of lead	
Borate of lead ?	
Prototartrate of tin	
Iodide of lithium.	135
15 Water	
Perchloride of copper	136
Acetate of soda cryst. (6 water)	
Citrate of barytes.	
Sulphate of zinc cryst. (water)	
Perphosphate of copper	
Bi-sulphate of potash crystallized (6 water)	137
Iodide of phosphorus	
——— magnesium	
Sulphate of cobalt (crystallized 7 water).	
Citrate of bismuth	138
Cryst. sulphate of iron (7 water)	139
Oxychlorate of potash	140
Chloride of lead	
Phosphate of lead	
Sulphate of nickel (crystallized 7 water).	
Arseniate of barytes	
Carbonate of silver	
Borate of silver ?	
Iodide of sulphur.	141
Arseniate of bismuth	142
Starch ?	143
Hydriodate of ammonia	
Sulphite of lead.	144
Columbium ?	
Peroxide of rhodium ?	
16 Water.	

Tartrate of barytes	145
Iodide of calcium	
Chloride of silver	146
Phosphate of silver	
Tartrate of bismuth	147
Benzoate of lime	148
Tungstate of lime	
Oxalate of lead	
Iodide of sodium	149
Cryst. nitrate of barytes (2 water)	150
Sulphite of silver	
Iodide of cobalt	151
Sulphate of lead	152
Benzoate of soda	
Bichromate of potash	
Columbic acid	
Carbonate of soda (crys. 11 water)	153
17 Water	
Chlorate of barytes	154
Benzoate of cobalt	
Iodide of nickel	
Oxalate of silver	
Benzoate of manganese	156
Sulphate of silver	157
Benzoate of nickel	
Borax (8 water)	158
Iodide of zinc	159
Persulphate of copper (dry)	160
Chloriodic acid	161
Crystallized sulphate of soda (10 water)	162
Succinate of lead	
Benzoate of zinc	
Acetate of lead	
18 Water	
Chromate of lead	164
Iodic acid	165
Iodide of potassium	
Nitrate of lead	166
Acetate of silver	168
Tungstate of potash	
Benzoate of potash	
Iodide of antimony	169
Iodide of strontium	
Chromate of silver	170
Citrate of lead	
19 Water	171
Nitrate of silver	172
Binarseniate of potash	
Arsenite of silver	
Arseniate of lead	174
Citrate of silver	176
Tartrate of lead	179
Binacetate of copper	180
Arseniate of silver	
20 Water	
Iodide of cadmium	181
Bi-tartrate of potash	182
Iodate of ammonia	
Malate of lead	
Iodide of tin	183
Acetate of copper (crys. 6 water)	184
Tartrate of lithia and soda	
Molybdate of lead	
Tartrate of silver	185
Pernitrate of copper (dry)	188
Chlorate of lead	
Protoxide of platinum ?	
Acetate of lead (cryst. 3 water)	189
Iodide of copper	
Molybdate of silver	190
Bitartrate of potash (cryst. 1 water)	191
Quadroxalate of potash	192
Iodate of lime	193
Chlorate of silver	194
Iodide of barium	195
Iodide of bismuth	197
Iodate of soda	
Tungstate of barytes	198
Benzoate of barytes	
Benzoate of bismuth	200
Mercury	
Gold	
Binacetate of copper (cryst. 3 water)	207
Iodate of zinc	208
Protoxide of mercury	
—— gold	
Potash-tartrate of ammonia	
Subacetate of copper	210
Phosphuret of mercury	212
Iodate of potash	213
Tartrate of soda and potash	214

Peroxide of mercury	216
Protosulphuret of mercury	
Prototartrate of iron and potash	218
Peroxide of gold ?	224
Iodide of lead	229
Benzoate of lead	232
Bi-sulphuret of mercury	
Iodide of silver	235
Protochloride of mercury	236
——— gold	
Tungstate of silver	238
Benzoate of silver.	
Iodate of baryta	243
Iodate of bismuth.	245
Sulphuret of gold ?	248
Protosulphate of mercury.	
Crystallized persulphate of copper (10 water)	250
By-cyanuret of mercury	252
Persulphate of mercury	256
Alum (dry)	262
Periodide of phosphorus	
Protonitrate of mercury	
Perchloride of mercury	272
Perphosphate of mercury	
Sub-binacetate of lead	274
Iodate of lead	277
Iodate of silver	283
Bi-persulphate of mercury	296
Chloride of gold and sodium	
Pernitrate of mercury	304
Protiodide of mercury	325
——— gold	
Crystallized chloride of gold and sodium (8 water)	369
Subtriatacetate of lead	386
Periodide of mercury	460
Alum (crystallized 25 water)	487

ARTICLE VIII.

Astronomical Observations, 1824.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Jan. 17.	Emersion of Jupiter's second satellite	6h 07′ 47″	Mean Time at Bushey.		
		6 09 08	Mean Time at Greenwich.		
Jan. 26.*	Jupiter's third satellite. In contact with the planet's limb. . .	10h 14′ 45″	M.T.Bushey.		
	Immersion.	10 25 10			
Jan. 29.	Emersion of Jupiter's first satellite	9 25 33	Mean Time at Bushey.		
		9 26 54	Mean Time at Greenwich.		
Jan. 31.	Emersion of Jupiter's second satellite	11 21 36	Mean Time at Bushey.		
		11 22 57	Mean Time at Greenwich.		
Feb. 1.	Jupiter's fourth satellite. Immer.	7h 48′ 35″	M.T.Bushey	7h 49′ 56″	M.T.Gr.
	Emer.	9 59 36		10 0 57	
Feb. 11.	Immersion of 2 α Gemini by the moon.	9 31 47·6	Siderial Time.		
Feb. 14.	Emersion of Jupiter's first satellite	7 43 56	Mean Time at Bushey.		
		7 45 17	Mean Time at Greenwich.		

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* According to the Nautical Ephemeris, the eclipse of this satellite took place at 13h 18′ 35″; but the satellite immerged behind the planet, at 10h 26′ 31″ Greenwich time, making a difference of 2h 52′ 04″.

ARTICLE IX.

On Animal Remains found in Caves. By G. Cumberland, Esq.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Bristol, Dec.* 29, 1824.

WHEN, in the *Annals* for February last, you did me the favour to publish my remarks on the enigmatical cave at Pickering, which has given rise to so many unsatisfactory conjectures, I confess I was carried away with the then prevailing opinion, that the remains of the animals found there had been localized to the spot, and their assembling there for safety was in that case not improbable at the rising of waters of the general flood. But I have since felt, that the objection founded on climate is insurmountable, and that they could not have been dragged there by the hyænas is now, I believe, the most general belief; for not to insist on what Dr. Knox asserted at the Wernerian Lectures, reported in the Physical Journal, No. 16, viz. that the hyænas of southern Africa are not in the habit of conveying their prey away into dens, it seems impossible to reconcile to reason that they should have found such various animals near together, as the elephant, rhinoceros, ox, horse, hippopotamus, tiger, bear, and wolf; or, if they had, that they should have been able to effect such a labour, so much beyond their united strength, or to have destroyed all the skulls by even their forcep jaws; neither can any one be made to believe, could all this be proved, that such animals as these antediluvian hyænas are described to be in point of magnitude, would have left even the smallest remains of such small bones as those belonging to the rat, mouse, raven, pigeon, and lark. Ducks and partridges would have been but a mouthful to them, and it is not very easy to imagine by what means they could be able to catch them any more than rabbits and hares.

Let us suppose the gnawed marks on the bones to be established by comparison; it proves nothing of their having been gnawed where they were found; and as to the polish acquired by their feet and hair passing over them, that really must always be considered as conjectural, and proves nothing as to locality.

Admit even that a considerable portion of original gelatinous matter (as has been asserted) remained, it could only show that the period since the destruction of these animals had not been very extended, not that they died there. Again, if no skull was left entire, but only *chips* found, and solid parts of bones, or angular fragments projecting through the stalagmite above, we should be a little cautious in naming so many species, and varieties of species, of small animals:—even Messrs. Cuvier and

Clift might startle at this difficulty one would think. But as this is a subject on which so few are competent to decide, and so much has been advanced on the continent by supposed infallible judges, that we must be silent on that head; only it does seem strange, that if some are so evidently gnawed, more are not so; and I do not find one in that supposed state among the immense mass that Mr. Cottle has collected from Oreston; and as some one must have been left if this was their den of slaughter, why have not *their* skulls been found—at any rate that of the last survivor? Stress has been laid on these bones not having been rolled, but I think that could not have been, or pebbles found with them, in the places where they are found; for if granite rocks have been proved to have been removed by water hundreds of miles, surely bones floating in masses might have been conducted by currents from very distant parts from those where we find them, and being lighter they would not have mixed with gravel or beach stones at the retreat of the waters, but probably washed up with mud, and entangled in each other, they fell into these cavities on the retreat of the waters, or were carried in by whirlpools, so as, after receiving many fractures, to be deposited at length in a level bed such as they were discovered in, resting on the original stalagmite, and when all was dry, receiving in the course of ages an additional covering of a similar deposition. This conjecture accounts also for their intermixture, as well as fractured state; for rolled bone, and wood, or ivory, are only invariably found among ancient beaches of gravel such as now lies below the land at Shorehampton, near Bristol, and which has produced many specimens whenever the mine is opened to gravel Lord de Clifford's park there, a deposit undoubtedly left there on the borders of the Severn channel at the retreat of the waters of the flood.

To come at the truth will be difficult, and therefore we are obliged to Prof. Buckland for the great pains he has taken in bringing forward all that has hitherto been known on a subject that seems so strongly to corroborate the Scripture history of the Deluge; but we must not in the history of natural events look to any authority but that which is founded on circumstances applicable to the event under discussion.

Yours, &c. G. CUMBERLAND.

Art. X.—*Tables of the Comparative Temperature of Pisa and Penzance.* By Mr. Edward Giddy.

1823. Day of the month.	Pisa.—Reaumur.			Fahrenheit.				Penzance.—Reaumur.			Fahrenheit.			
	8 a. m.	noon.	11 p. m.	8 a. m.	noon.	11 p. m.	Mean.	8 a. m.	2 p. m.	8 p. m.	8 a. m.	2 p. m.	8 p. m.	Mean
Jan. 1	−0·8	−1·7	−0·5	30·2	28·2	30·8	29·7	+3·5	+6·2	+3·5	39·8	45·9	39·8	41·8
2	1·2	+3·0	1·0	29·4	38·7	29·8	32·6	5·3	8·0	7·1	43·9	50·0	47·9	47·2
3	0·3	4·5	2·5	30·8	42·1	26·4	33·1	7·1	8·0	7·1	47·9	50·0	47·9	48·6
4	0·0	3·0	0·1	32·0	38·7	31·8	34·4	7·1	8·0	7·1	47·9	50·0	47·9	48·6
5	0·3	2·7	0·0	31·4	38·0	32·0	33·8	5·3	8·0	5·3	43·9	50·0	43·9	45·9
6	+1·0	4·0	2·3	34·2	41·0	37·1	37·4	3·5	6·2	3·5	39·8	45·9	39·8	41·8
7	2·0	4·6	2·1	36·5	42·3	36·7	38·5	4·4	6·2	3·5	41·9	45·9	39·8	42·5
8	2·0	4·0	2·1	36·5	41·0	36·7	38·1	5·3	5·3	5·3	43·9	43·9	43·9	43·9
9	2·0	5·0	3·0	36·5	43·2	38·7	39·5	5·7	5·7	5·7	44·8	44·8	44·8	44·8
10	3·0	5·0	4·0	38·7	43·2	41·0	41·0	4·4	4·4	1·7	41·9	41·9	35·8	39·9
11	5·0	5·7	5·0	43·3	44·8	43·2	43·7	0·0	2·6	0·8	32·0	37·8	33·8	34·5
12	3·7	4·2	2·8	40·3	41·4	38·3	40·0	−0·8	0·8	−0·8	30·2	33·8	30·2	31·4
13	4·2	5·2	2·3	41·4	43·7	37·1	40·7	+0·8	2·6	+1·7	33·8	37·8	35·8	35·8
14	3·3	3·4	2·0	39·4	39·6	36·5	38·5	2·6	2·6	0·0	37·8	37·8	32·0	35·9
15	−0·6	4·0	3·1	30·7	41·0	38·9	36·9	0·4	0·8	−2·2	31·1	33·8	27·1	30·7
16	+5·8	7·0	6·4	45·0	47·7	46·4	46·4	0·8	1·7	0·8	30·2	35·8	30·2	32·1
17	6·7	4·4	5·0	47·0	41·9	43·2	44·0	0·8	1·7	0·8	30·2	35·8	30·2	32·1
18	5·1	5·0	4·2	43·4	43·2	41·6	42·7	0·8	1·7	0·8	30·2	35·8	30·2	32·1
19	3·8	4·2	3·0	40·5	41·4	38·7	40·2	+0·8	1·7	0·0	33·8	35·8	32·0	33·9
20	2·3	4·0	2·9	37·1	41·0	38·5	38·7	0·8	1·7	+0·8	33·8	35·8	33·8	34·5
21	2·6	4·1	1·0	37·8	41·2	34·2	37·7	−0·4	0·4	0·0	31·1	32·9	32·0	32·0
22	0·0	2·6	1·1	32·0	37·8	29·6	33·1	+0·4	1·3	0·8	32·9	34·9	33·8	33·9
23	0·1	3·0	+3·0	32·2	38·7	38·7	36·5	0·0	0·8	0·0	32·0	33·8	32·0	32·6
24	3·0	4·0	1·3	38·7	41·0	34·9	38·2	0·4	1·7	1·7	32·9	35·8	35·8	34·8
25	2·7	4·1	5·2	38·0	41·2	43·7	41·0	3·5	4·4	3·5	39·8	41·9	39·8	40·5
26	7·3	7·2	2·8	48·4	48·2	38·3	45·0	2·6	3·5	2·6	37·8	39·8	37·8	38·5
27	3·2	4·3	5·7	39·2	41·6	44·8	42·0	8·0	9·7	8·0	50·0	53·8	50·0	51·3
28	5·3	7·0	7·3	43·9	47·7	48·4	46·7	7·1	8·0	7·1	47·9	50·0	47·9	48·6
29	7·2	9·2	8·3	48·2	52·7	50·6	50·5	7·1	8·0	7·1	47·9	50·0	47·9	48·6
30	8·3	9·5	7·2	50·6	53·3	48·2	50·7	6·2	7·1	6·2	45·9	47·9	45·9	46·6
31	5·0	8·0	7·0	43·3	50·0	47·7	47·0	6·2	8·0	7·1	45·9	50·0	47·9	47·9
	Min. −1·2	Max. +9·5	Min. −2·5	Min. 29·4	Max. 53·3	Min. 26·4	Mean 39·9	Min. −0·8	Max. +9·7	Min. −2·2	Min. 30·2	Max. 53·8	Min. 27·1	Mean 39·9

The above tables clearly show, that in severe winters the temperature at Pisa (which is a place of great resort for invalids) and at Penzance is as near as possible the same. The Thermometrical Register at Pisa was kept at the Observatory, 6 miles E.S.E. from the city.

Article XI.

An Account of the Volcanos at present in Activity. By M. Arago.*

Some persons having appeared desirous of seeing in the "Annuaire" an account of the volcanos now in activity, I engaged to write it, but without having sufficiently reflected, as I afterwards discovered, on the difficulty of this work. The details with which most travellers have furnished us on these great phænomena are incomplete, and extremely vague. In the estimation of one, those parts of the earth from which a little smoke arises, or upon which a few sparks are perceived, are volcanos; another gives this name only to mountains which incessantly cast forth torrents of lava, burning matter, and ashes. The first will insert in his catalogue the trifling flames of Pietra-Mala, Barigazzo, Velleia, of Persia, and Caramania; the second will place Santorini itself in the class of solfaterras. To this difficulty must be added the still greater one of determining what distance should separate two craters, that they may be considered as two distinct volcanos. At Teneriffe the eruption of 1706 broke out at an opening two leagues distant from the Peak; that which destroyed Garachico burst out at an opposite side, and at a point a league and a half distant from the Peak; there were then three leagues and a half between the two openings without their being considered as belonging to two distinct volcanos. But, shall we consider the isle of Palma, where there was an eruption of lava in 1699, as containing a volcano separate from that of Teneriffe? Ought the destruction of a third of the isle of Lancerote in 1730 to be considered as the effect of a lateral eruption of the volcano of the Peak, or of a separate volcano? Analogous questions present themselves at every step, and the means are wanting to answer them. I should, therefore, have omitted printing this notice in the *Annuaire*, from which it is desirable to exclude every thing that does not possess a certain degree of precision, if I had not had the advantage of consulting the two persons to whom the physical history of the globe is best known, MM. de Humboldt, and Leopold de Buch.

Volcanos of Europe and the adjacent Islands.

Vesuvius; kingdom of Naples.
Etna; Sicily.
Stromboli; Eolian islands.
Hecla; Iceland.
Krabla; northern part of Iceland.
Katlagiaa-Jokul; Iceland.

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* From L'Annuaire pour l'an 1824.

Eyafialla-Jokul; Iceland, south-east of Hecla.
Eyrefa-Jokul; ditto.
Skaptaa-Jokul; ditto.
Skaptaa-Syssel; ditto.
Wester-Jokul; ditto.
Esk; island of Jean Mayen.

Vesuvius, the only volcano now in activity upon the continent of Europe, has been repeatedly extinguished, and in combustion. Before the reign of Titus, this mountain was much visited, and is mentioned only on account of its extraordinary fertility. Vitruvius and Diodorus Siculus, who lived in the time of Augustus, do indeed state, upon historical authority, that Vesuvius had formerly vomited fire like Etna; but these statements refer to remote and nearly forgotten periods.

It was on the 24th of August in the 79th year of the Christian era, that Vesuvius was rekindled. This eruption buried the cities of Herculaneum, Pompeii, and Stabiæ; and it will be recollected that Pliny, the naturalist, perished, as the victim of the ardent curiosity with which he was inspired by this interesting phænomenon.

After the eruption of the year 79, the volcano remained in combustion for 1000 years; still later it appeared to be totally extinct; so that in 1611, the mountain was inhabited almost to the summit, and there were a copse and small lakes in the interior of the crater.

Etna.—Pindar, who lived in the year 449 before the Christian era, mentions Etna as being in a state of combustion. Thucydides has preserved some details of the eruption which occurred 476 years before Christ. As to Homer, he does not even mention the mountain, although in the Odyssey he disembarks Ulysses in Sicily. The silence of a poet who has always been admired for the extent and universality of his knowledge, has led to the probable supposition, that long before the time of Homer the volcano was extinct. The Roman historians, both of the middle ages and of modern times, have described so great a number of eruptions of Etna, that it probably would not be difficult to prove that during a period of 2000 years it was never extinct for a whole century.

Seneca has observed that volcanic mountains do not supply the combustible matter of the fire, but that they merely give it vent. Father Kircher seems to have commented on these words of the Roman philosopher, when in the fourth book of his Subterranean World, he has advanced the opinion that the matter ejected from Etna, would, if formed into one mass, form a mountain 20 times larger than Etna itself. The work of Father Kircher appeared in 1660. Nine years afterwards a single eruption of the volcano, covered with lava a space of six leagues long, two and a half in width, and of a mean depth of at least

100 feet. According to Dolomieu the eruption of 1755 produced a current of lava four leagues in length, half a league wide, and of at least 200 feet mean depth. In reflecting on the immense void, that eruptions so considerable must have produced in the mountain and at its base, is there not yet cause for wonder that eruptions, like that of 1787 for instance, should still occur at the summit, the height of which is 10,600 feet above the level of the sea.

Stromboli.—M. de Humboldt has remarked that the activity of volcanos appears to be in the inverse ratio of their magnitude. Stromboli is a striking confirmation of this principle; it is perpetually sending forth flames; but with this peculiarity, that for 2000 years it has not, strictly speaking, made any eruptions, although the nature of the surrounding country shows, that it was formerly subject to them. Mount Epomea, in the island of Ischia, ought not to be considered as a volcano, but it would probably become one if Stromboli were extinct.

Santorini was the site of a great eruption in 1707. As this phænomenon has not been repeated, and as the island exhibits no crater, of the true chimney of a volcano, I have not inserted it in the catalogue.

Volcanos in Iceland.—The last eruption of *Hecla* occurred in 1766. The eruptions of this volcano, according to Sir George Mackenzie, are not in general so extensive as they have been represented. The most recent eruption of *Krabla* occurred in 1724. In 1756, between January and September, there were five eruptions of *Kattlagiaa*; since which period this volcano remained perfectly tranquil, until the 26th of July, 1823, when strong eruptions occurred accompanied with earthquakes.

Eyafialla-Jokul, which appeared to be extinct for more than a century, emitted torrents of flames from its summit on the 20th of Dec. 1821. Eye-witnesses report that the column of fire was still visible on the 1st of Feb. 1822, and that it projected stones weighing from 50 to 80 pounds, with so much force as to cause them to fall at a distance of two leagues from the mountain. The mountain burst at its base on the 26th June, 1822, and a great quantity of lava issued from it.

Eyrefa-Jokul.—The last eruption occurred in 1720

Skaptaa-Jokul and *Skaptaa-Syssel.*—The eruptions of these two volcanos, which occurred in 178 , occupy the first rank in phænomena of this nature; they ravaged an immense extent of country. During a whole year after the eruptions, the atmosphere of Iceland was mixed with clouds of dust, which the sun's rays scarcely penetrated.

Wester-Jokul.—An eruption of ashes and stones happened in January, 1823.

Esk.—This volcano was discovered and visited in 1817 by Mr. Scoresby. It made an eruption at the end of April, 1818;

columns of smoke arose every three or four minutes to the height of 4 or 5,000 feet.

Islands near the Continent of Africa.

No volcano, strictly so called, is with certainty known to exist in Africa; but the islands which geographers consider as the dependancies of that continent, contain several volcanos.

El Pico.—Island of El Pico, Azores.
Peak of Teneriffe.—Island of Teneriffe.
Fuego.—Island of Fuego, Archipelago of Cape Verd.
Les Trois Salasses.—Isle of Bourbon.
Zibbel-Teir.—Island of this name, Red Sea.
Ascension Island.

El Pico.—This mountain is the only one of the Azores which rises in the form of a cone; the only one entirely composed of trachyte, and the only one in which there is a vent always open. Geologists are agreed in the opinion, that the great currents of lava which flowed in 1812 in the Isle of St. George were the results of a lateral eruption of the volcano of El Pico. They explain in the same way the sudden formation of an isle in the neighbourhood of St. Michael in 1811. This isle was taken possession of in the name of the King of England, by the Captain of the Sabrina, who witnessed the event; it has since totally disappeared. The part of the sea in which this isle arose is not less than 80 fathoms deep.

Peak of Teneriffe.—This volcano appears to be much more agitated on its sides than at its summit. Neither flames nor lava have issued from it from time immemorial, nor any smoke which could be seen at a distance. The last eruption, that of 1798, took place laterally in the mountain of Chahorra. It continued for more than three months. Various fragments of rocks, of very considerable size, which the volcano projected from time to time into the air, occupied, according to the observations of M. Cologuan, from 12 to 15 seconds in falling. Teneriffe had suffered no eruption for 92 years, until that of 1798, which began suddenly on the 9th of June.

Immense torrents of lava flowed upon the island of Palma, 25 leagues distant from the Peak, through new volcanic openings which were formed in 1558, 1646, and 1677. The isle of Lancerote was also destroyed by an eruption in 1730.

Fuego.—Scarcely any details are known respecting the isle of Fuego. It would appear, in opposition to an opinion formerly adopted, that no other active volcanos exist in all the Archipelago of Cape Verd.

Volcano of Bourbon.—There are few volcanos which are in a state of greater activity than that of Bourbon. Its last eruption occurred on the 27th of Feb. 1821. It formed three currents of lava, which opened a passage in the summit of the mountain, a little below the true crater. One of these currents did not reach the sea till the 9th of March. Some time after the explosion, there fell in many parts of the island, a shower composed of black ashes, and long flexible threads of glass, resembling golden-coloured hair. This phænomenon, which was chiefly noticed in 1766, has been considered as peculiar to the volcano of Bourbon; but Hamilton states, that he found similar glassy filaments mixed with the ashes by which the atmosphere of Naples was obscured during the eruption of Vesuvius in 1779.

Those persons who have not particularly studied volcanic phænomena will probably be surprised to learn, that in 1821 the ignited lava of the volcano of Bourbon should be six whole days in traversing, upon inclined ground, the short distance from the crater to the sea. But it ought to be observed that lavas are not perfect fluids, and that in proportion as they cool, their progress must slacken. In 1805, M. de Buch observed a torrent of lava issue from the summit of Vesuvius, and reach the sea shore in three hours; but the history of volcanos offers few instances of similar rapidity.

In general the motion of lavas is slow; those of Etna are whole days in flowing a few feet in the flat lands of Sicily. The external part is sometimes fixed and stationary; while the central mass, still fluid and incandescent, continues to flow. The great viscidity of the lavas, when slightly cooled, occasions them to be extremely thick on the edges even when they flow in a level country.

Zibbel-Tier, according to Bruce, is in 15½ degrees north latitude. The summit of the mountain has four openings, through which there issue thick columns of smoke.

Few details are known respecting the volcano of Ascension Island. As to that of Madagascar, which is stated to project immense columns of aqueous vapour visible at a distance of 10 leagues, its existence has not appeared to me sufficiently proved to induce me to insert it in the catalogue.

VOLCANOS OF AMERICA.

North-west Coast.

Mount Saint-Elia.
Mount del Buen Tiempo.
Volcan de las Virgenes?

Mexico.

Orizaba or Citlaltepetl.
Popocatepetl or volcan de la Puebla.
Tuxtla.
Xorullo.
Colima.

Guatimala and Nicaragua.

Volcano of Soconusco.
Sacatepeque.
Hamilpas.
Atitlan.
Fuegos de Guatimala.
Acatinango.
Sunil.
Toliman.
Isalco.
Sacatecoluca, near the Rio del Empa.
San-Vicente
Traapa.
Besotlen.
Cocivina, near the gulf of Conchagua.
Viego, near the port of Rialexo.
Momotombo.
Talica, near San-Léon de Nicaragua.
Granada.
Bombacho.
Papagallo.
Barua, south of the gulf of Nicoya.

Volcano of Sotara.
Purace. } Group of Popayan.
Pasto.
Rio Fragua.

Volcano of Cumbal.
Chiles.
Azufral. } Group of the province de los Pastos.

Group of Quito.

Volcano of Antisana.
Rucupichincha.
Cotopaxi.
Tunguragua.
Sangay.

Volcano of Arequipa (Peru).

Group of Chili.

Volcano of Copiapo.
Coquimbo.
Choapa or Lisuari.
Aconcagua.
Santiago.
Petoroa.

Chillan.
Tucapel.
Callaqui.
Chinal.
Villa-Rica.
Votuco.
Huaunauca.
Huaiteca.
San-Clemente.

Antilles.

Volcano of Saint-Vincent.
Saint-Lucia.
Guadaloupe.

It is unknown whether the volcanos of the north-west coast have recently made any eruption.

Orizaba is 17,300 feet high; the streams of lava observed on the sides of the mountain remove every doubt as to its volcanic nature; but no recent eruptions are known of.

Popocatepetl has smoked ever since the conquest of Mexico. Cortes relates indeed, that he ordered ten of his most courageous companions to reach the summit, and to discover the secret of its smoking, which he wished to communicate to Charles V. This volcano is always burning, but it has projected lava from time immemorial. Its height measured by M. de Humboldt is 17,600 feet.

The volcano of *Tuxtla* is situate to the south-west of Veracruze. Its last very considerable eruption occurred in 1793. The ejected ashes were then carried as far as Perote, a distance, in a straight line, of 57 leagues.

Xorullo.—M. de Humboldt remarks, that the catastrophe which gave rise to the volcano of Xorullo, is, perhaps, one of the most extraordinary physical revolutions which the annals of our planet contain. In the middle of a continent, at 36 leagues distance from any active volcano, the earth rose to the extent of three or four square miles in the form of a bladder, on the night of the 28th and on the 29th of September, 1759. In the centre of a thousand inflamed cones, six mountains from 1,300 to 1,700 feet high above the original level of the surrounding country, suddenly arose. The principal of them is Xorullo, the height of which is 1,700 feet. Its eruptions continued without cessation until the month of February, 1760. The subterranean fire is now very active.

The volcano of *Colima*, the most western of those in New Spain, ejects now hardly any thing but ashes and smoke. Its height is about 1,000 feet.

M. de Humboldt has made the important observation, that the Peak of Orizaba, Popocatepetl, Colima, and other extinct volcanos, are in a line, as if they proceeded from one fissure or vein, in a direction perpendicular to that of the great chain of mountains which traverses Mexico from the north-west to the south-east.

The volcano of Xorullo just mentioned interposed itself in 1759 in the line of the ancient volcanos. This curious arrangement, which we shall observe in other places, exists also, according to M. Daubuisson, in the extinct volcanos of Puy-de-Dôme.

The volcanos of *Guatimala* which have most lately erupted, are Los Puegos of Guatimala, Isalco, Momotombo, Talica, and Bombacho. These active volcanos, and the sixteen others whose names have been mentioned, are contained between the 10° and 15° of north latitude, and in a line corresponding with the general direction of the Cordilleras.

The connexion of the volcano of *Pasto* with those of the province of Quito was shown in a striking manner in 1796. A thick column of smoke had existed from the month of November, 1796, from the volcano of Pasto; but to the great surprise of all the inhabitants of the city of that name, the smoke suddenly disappeared on the 4th of February, 1797. This was precisely the moment at which, at 65 leagues further south, the city of Riobamba, near Tunguragua, was destroyed by a tremendous earthquake.

Antisana is 20,000 feet high. No eruption of this volcano is known to have happened since the year 1590.

The last eruption of *Rucupichincha* occurred in the year 1660.

Cotopaxi made an eruption in 1742, while the French academicians were measuring a degree of the meridian in its neighbourhood. The column of flames and of burning substances rose 500 toises above the mountain. The snows which had been heaped up during two centuries, from the summit of the mountain to 500 toises below it, were melted *en masse*; the torrent which it occasioned rushed into the plain with impetuosity, forming waves from 60 to 100 feet in height. At a distance of three or four leagues from the mountain, the rapidity of the water, in the opinion of Bouguer, was from 40 to 50 feet in a second. Six hundred houses were destroyed, and 700 or 800 persons were drowned in the torrent. The eruptions of 1743 and 1744 were still more disastrous.

Bouguer and La Condamine, having examined the remaining traces of the great eruption of 1533, the memory of which is preserved from generation to generation among the inhabitants, they found that the volcano had ejected to a distance of more than three leagues, stones containing from 70 to nearly 100 cubic feet, or to use the expression of La Condamine, larger than the cabin of an Indian. The origin of these stones was unquestionable; they formed lines in every direction towards the volcano. It does not appear that Vesuvius has ever ejected stones to more than 4,000 feet high.

Tunguragua made an explosion in 1641.

Sangay has remained constantly burning ever since the year 1728.

Chimborazo does not appear in the list; for although no one disputes its volcanic nature, no account of its eruption has been preserved. The case is the same with *Carguairazo*. The inundation of mud which in 1698 covered 18 square leagues of land was not the effect of an eruption, properly so called. When Carguairazo fell, the waters which it concealed in its bosom were precipitated impetuously into the plain, and occasioned the disasters mentioned by the historians of America.

There are in some maps of Chili more volcanos marked than I have placed in the catalogue; but I felt it proper to confine myself to what appeared to me to be most certain; and I ought further to add, that of the sixteen volcanos of this country whose names have been given, several are now probably extinct, *Peteroa* made an eruption in 1762; *Villa-Rica* in 1640, &c.

In looking at the coast of America, it will undoubtedly have occasioned surprise to find no volcano, either between the 2d and 16th degree of south latitude, or between the 17th and 27th degree. If the volcano of Arequipa did not exist, the range of Guatimala and Nicaragua, the groups of Popayan and los Pastos would be separated from the long track of Chili, by a space of 25° of latitude, totally without volcanos. Although Peru contains only one volcano, there are few countries in the world where earthquakes are more severely felt, and where they make greater devastation. They frequently occasion the formation of immense fissures, over which bridges are built to preserve communication between different provinces. One of these fissures, after the earthquake of 1746, was a league in length, and nearly seven feet wide.

The volcano of the island of St. Vincent ejected lavas in 1718 and 1812. The ashes of the latter eruption were carried by the upper counter current of the trade winds, to the island of Barbadoes, 30 leagues to the west.

At St. Lucia, there is a continual formation of sulphur, occasioned by the condensation of the vapours, which rise from the crater called *Oualibou*, at a height of 1200 to 1800 feet. Jets of hot water are also observed there.

The volcano of Guadaloupe, at a height of about 4,800 feet, made its last eruption in 1797. It then ejected pumice, ashes, and clouds of sulphureous vapours.

I shall conclude these notices relative to the volcanos of America, by remarking, that no active volcanos occur either at Buenos Ayres, at Brazil, Guyana, or on the coast of Venezuela, or in the United States; that is to say, at any point of the coast to the east of this great continent. There exist to the east of the Andes only three small volcanos situate near the sources of the

Caqueta, the Napo, and the Morona, and which, according to M. Humboldt, probably result from the lateral action of the volcanos of Popayan and Pasto.

Volcanos of Asia.

Elburs, in Persia.

Tourfan, central region of Asia; latitude 43° 30′; longitude 87° 11′.

Bisch-Balikh.—Ibid. Latitade 46° 0′; longitude 76° 11′.

Avatscha.—Kamtschatka.

Tolbatchick.—Ibid.; and three other volcanos more considerable than the two last.

Kourile Islands.

Nine active volcanos, according to Kracheninnikou.

Aleutian Islands.

Four volcanos at Ouminga, Ounalaska, Omnak, and Ourimack. The last made a great eruption in 1820.

Islands of Japan.

Ten volcanos. The island of Niphon, which is the most extensive, contains three. According to the evidence of Kœmpfer, several of the volcanos of Japan are subject to very violent eruptions.

Islands of Lieou-Kievu.

The Sulphur Island emitted a thick sulphureous smoke, when the Lyra, commanded by Capt. Basil Hall, passed near it on the 13th of Sept. 1816.

Elburs has been mentioned by several travellers as a volcano in activity; but the fact is doubtful, and at any rate there is no evidence to prove that it has recently made any eruption.

The mountains of *Tourfan* and *Bisch-Balikh* are represented as continually emitting flames and smoke. It is stated that the Kalmucks collect sal ammoniac there, which they export to the different countries of Asia.

Avatscha made an eruption in 1779, while Capt. Clerke was in the harbour of St. Peter and St. Paul. In 1787 La Peyrouse and his companions saw flames and smoke continually at the summit of the same mountain.

An eruption of *Tolbatchink* occurred in 1739. A third volcano, and more considerable than the two others, but of which Capt. Clerke does not give the name, ejected a permanent column of smoke from its summit. Since this, two new volcanos have made eruptions at Kamtschatka.

OCEANIA.

Philippine Islands.

Five active volcanos. Travellers have hitherto given only vague accounts of the volcanos of the Philippines. *Albay* is the name of that in the island of Luconia; *Taal* is situate to the south of Manilla; *Fuego* to the south of Luconia; *Mindanao* also contains a volcano.

Borneo.

Geographers agree in assigning volcanos to Borneo, but without stating either their number or situation with precision.

Barren Island.

Barren Island contains a very active volcano of nearly 4000 feet high, which frequently ejects immense columns of smoke, and red-hot stones, of the weight of three or four tons. Its latitude is 12° 15′. Its distance from the most eastern of the Andaman Islands is 15 leagues; the island is not more than six leagues in circumference.

Sumatra.

Four volcanos are marked by Marsden in his map of Sumatra; but as the interior of the island is very little known, there probably exist a greater number.

Java.

The island of Java contains a great number of volcanos arranged in right lines; their names and the dates of their eruption are the following:

Salak, 1761; eruption.
Tankuban, 1804; sulphureous vapours.
Guntur, 1807; eruption.
Gagak, ——; partial combustion.
Chermai, 1805; eruption.
Lawn, 1806; sulphureous vapours.
Arjuna, ——; permanent column of smoke.
Dasar, 1804; eruption.
Lamongan, 1806; eruption.
Tasher, 1796; eruption.
Klut, 1785; eruption.

Arjuna is 10,614 feet high; this mountain is not, however, the most lofty in the island.

Mount Papandayang was one of the principal volcanos of the island; but it is no longer in existence. Between the 11th and 12th of August, 1772, after the formation of a great luminous cloud, the mountain totally disappeared in the bowels of the earth. It has been estimated that the land thus ingulphed was 14 miles long and 6 miles broad.

Sumbawa.

Tomboro, in Sumbawa, made a violent eruption in 1815. The detonations were heard in Sumatra at places 300 leagues distant from the volcano in a right line.

Flores.

The volcano of this island was seen by Bligh.

Daumer.

Daumer contains a volcano.

Dampier, in 1699, saw a volcano constantly in combustion on a small island between Timor and Ceram.

Island of Banda.

Goonoung-Api, in Banda, made a violent eruption on the 11th of June, 1820, during which it ejected red-hot stones as large as the habitations of the natives. Several of these stones rose to a height double that of the mountain.

Moluccas.

In the island of Ternate, there is a burning volcano. Tidore is the name of one of these islands, and of an active volcano which it contains.

According to geographers, Celebes contains several active volcanos; they do not mention their situations.

Sanguir.—Between Mindanao and Celebes, is one of the greatest volcanos of the globe.

New Guinea.

Two volcanos were burning, in 1700, in the island of New Guinea, when Dampier explored the coast of it.

New Britain.

There are three volcanos in the Archipelago of New Britain. D'Entrecasteaux saw an eruption of that which is situated in latitude 5° 32′, and 145° 44′ of east longitude, the 29th of June, 1793. A torrent of lava flowed into the sea, and formed different cascades. Lemaire and Schouten formerly saw an eruption of the same volcano.

The Archipelago of l'Espiritu Santo.—The island of Amhrym, in this Archipelago, which Bougainville called the Great Cyclades, and Cook the New Hebrides, contains an active volcano. That of Tanna is also volcanic. In Aug. 1774, Cook witnessed one of its eruptions. The volcano cast forth flames, ashes, and stones of a size at least equal to that of the great boat belonging to his ship. In April, 1793, d'Entrecasteaux and his companions saw a thick column of smoke on the top of the mountain.

Archipelago of the Ladrones.

There are nine volcanos in this Archipelago; but I do not know if they are all to be placed in the class of those which are still burning.

Sandwich Islands.

The Mouna-Roa, in Owhyhee, appears to be, or at least to have been a volcano; but is it the same as the mountain of Mowee, which Vancouver has called the Volcanic Mountain.

The Island of Amsterdam.

The island of Amsterdam was burning when D'Entrecasteaux saw it in the month of March, 1792. Some attribute this phenomenon to the effect simply of a great fire; others have concluded that the island contains a volcano.

The Islands of the Marquis de Traversé.

The islands lately discovered by the Russian navigators, between New Georgia and Sandwich Land, contain an active volcano. There exists one equally so in Sandwich Land.

General Summary.

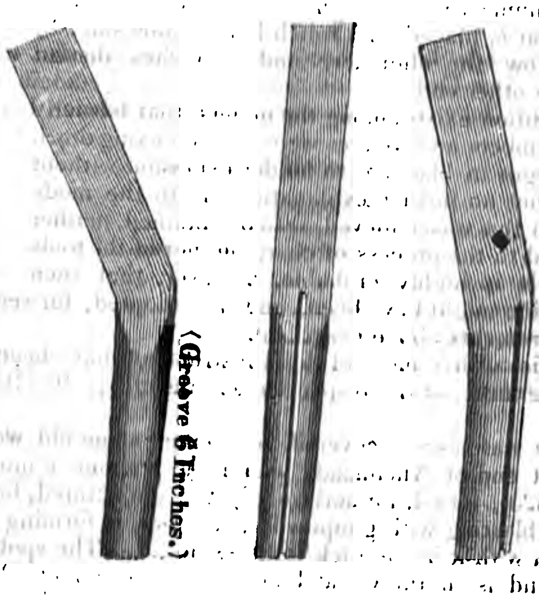
	Number of active volcanos.		
	On the Continent.	In the Isles.	Total.
Europe	1	11	12
Africa	0	6	6
America	58	3	61
Asia	8	24	32
Oceania	0	52	52
	67	96	163

Before I finish this account, I shall remark, that if the two volcanos in the central part of Asia are excepted, the existence of which may appear doubtful, not one will be found in the preceding list which is more than 50 leagues from the sea. It seems difficult not to draw the conclusion from this curious fact, that water acts an important part in volcanic eruptions.

A phenomenon equally worthy the attention of observers, is the propagation of sound which precedes or accompanies eruptions. It has been previously shown, that in 1815 the explosions at *Tomboro,* in Sombrero, were heard at Sumatra, distant in a right line 300 leagues from the mountain. M. de Humboldt states in his excellent work a circumstance nearly as surprising. The explosions which announced the first eruption of ashes from St. Vincent, did not appear louder to the inhabitants of the island than the report of a large cannon. These explosions notwithstanding were heard perfectly upon the Rio-Apure, at the confluence of Rio Nula, 200 leagues from the volcano, which is equal to the distance of Vesuvius from Paris. The report seemed so well transmitted by the air, that it was mistaken for the discharge of artillery, and was the cause of several military movements in various parts of the American continent.

ARTICLE XII.

An Account of certain Instruments formerly used for the Purpose of Blasting in the Lead Mines of Colonel and Mrs. Beaumont, at Allenheads. Communicated by Mr. Thomas Crawhall, of Newcastle-upon-Tyne.*



THESE sketches represent an iron instrument found in Allenheads lead mines, supposed to have been formerly used in blasting, the length of which was $2\frac{1}{2}$ or 3 feet; the upper part having since been cut off, there only now remain 6 inches above the bended part, which is $1\frac{1}{4}$ inch square to the elbow, forming an angle of about 10°; is of a cylindrical shape, slightly tapering to the other end, which is one inch in diameter. On the outward side of the angle, along the circular part, is a groove six inches in length, of one-quarter inch broad, and of similar depth, projected (it is supposed) to receive the train of gunpowder, pertaining to the charge: the application of which has been to drive it tightly into the hole bored in the rock above the powder, and the upper part fixed by strong timbers placed across the top for the purpose of preventing it being thrown out, without the desired effect.

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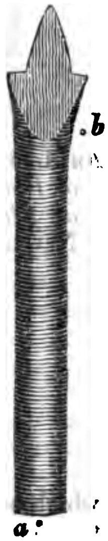
* From the Archæologia Æliana, vol. i.



Another instrument of iron, found in the same lead mines, differs from the above, in wanting the square bar at top, and in place of the hollow on one side, is cylindrical, and has a tube, one inch diameter, to nearly the upper end, where it is flattened, and has a shoulder projecting half an inch on each side, resembling the head of a spear, and apparently intended for fixing across it bars of iron or timbers, to oppose the violence of the ignited gunpowder.

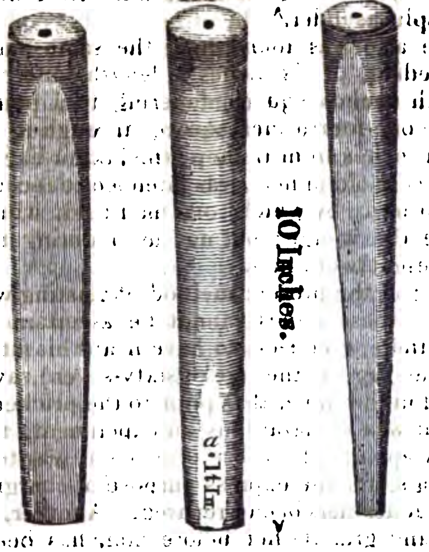
At the round end of the cylinder is a perforation *a*, communicating through the hollow tube, with another at *b*, placed for a touch hole on one side, 1¼ inch below the shoulder, and 8 inches distant from the other end.

A tradition exists among the miners, that formerly strong timbers and wedges were used for fixing down the charges in blasting, to hinder explosion without effect; but no further explanation, as to the mode in which this was achieved, is to be obtained, neither in regard to the process of charging, nor of the tools used. It is highly probable, however, that such application might have been, and was adopted, for securing the two instruments above described.

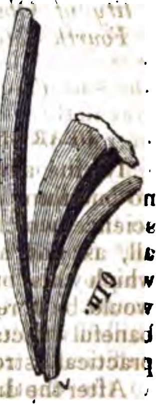
A series of five more of these instruments have been found in the same mine, of the respective lengths of 8¼, 10, 10¼, and 12 inches.

There was also discovered, in opening some old workings at the west end of Allenheads lead mines, about a month since (Jan. 1820), a tool, formerly used, it is conjectured, for the purpose of blasting with gunpowder, or rather, in forming a communication with it in the rock to be exploded. The spot where it was found is in the Great Limestone there, about 40 feet from the surface. The latest record of this place having been wrought, was in the year 1716, since which period this part of it has been entirely filled up with rubbish and fallings in of the vein, and only recently re-opened; when the following (see next page), with some other instruments, were discovered in one of the flatts in the limestone. The oldest wotkmen of the present day do not recollect their use, nor did they ever hear of such tools employed for the purpose; they seem, however, to have been meant for it, and their application as follows:—After having drilled a hole in the rock to be blasted, with a chisel or jumper sufficiently deep, the gunpowder is put into the bottom of it, say to the depth of three or four inches; next the tool sketched one side and flat on the other, as this hole, the former being next to the rock, [illegible] was driven between and a portion of a split [illegible]

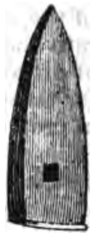
[illegible]



which is round at one end, one inch in diameter, with a hole in the centre about one-eighth of an inch, which communicates with another of the same dimensions, about one and one-fourth inches from the other end on the cylindrical side, the opposite being flattened from within one inch of the bottom, or circular end, to one-third of an inch thick at the other extremity; this hollow cavity appears to have been filled with powder, which, when the instrument was placed in the hole, would immediately communicate with the charge. In this situation, it is presumed, wedges (of wood) were driven against the flat side of the iron tube, to resist the force of the gunpowder, when fired through the touch-hole marked *a*, by a train or match laid for that purpose. How long this has been in disuse is altogether uncertain, even the name is forgotten: it is probable a century might since have passed away.



Nearly in the same spot with the above, to which I annex a sketch, a tool of more recent use was found, called by the miners the stock and feathers; and remembered by some to have been occasionally used about fifty years ago, particularly in wet situations, where gunpowder could not, without great difficulty, be applied. A perforation was made in the stratum, say four to six inches deep; placing two thin pieces of iron, called the feathers, which are rounded on one side and flat on the other, in this hole, the former being next to the rock, the wedge or stake was driven between until a portion of it split asunder.



This wedge also was found near the same place with the preceding, of six inches in length, and one and one-fourth inches square, tapering to a point, having a hole one-fourth inch square, through it, at one and a half inches from the top; this, according to the reports of very old miners, was intended to receive a small rod of iron, by which, one man held, whilst another drove the wedge; but not used during the life of any present workman.

At what period the present method of blasting was introduced into these mines cannot be ascertained. A person now residing there, recollects to have heard his father (who died thirty-nine years ago at the age of sixty-seven) say, although it took place before his time, that prior to the pricker and drive-all being used, it was so hazardous an experiment, that two men were specially appointed, whose province it was to visit the different workings, for the express purpose of charging and blasting, after the holes had been prepared. Another, who, as well as his father and grandfather before him, has been a pickman for sixty years past, has a faint remembrance of hearing very old men say, that formerly stemples were employed, but has no knowledge as to the process, nor even saw any other mode practised than the present; but that the stock and feathers had been in use during both the lifetimes of his father and grandfather.

Article XIII.

Inquiry how far the Opinions generally entertained of the Inutility of Observations of the Eclipses of Jupiter's Third and Fourth Satellites, are well or ill founded. By J. South, FRS.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Blackman-street, Feb. 23, 1824.*

To the advancement of natural knowledge there is probably no one thing so inimical as prejudice, and perhaps there is no science which has suffered so much from this common enemy to all, as has astronomy. To enumerate the various mischiefs which this busy fiend has inflicted upon this peculiar science would be foreign to the present purpose. Suffice it to say, its baneful effects have been not only felt by physical, but also by practical astronomy.

After the discovery of telescopes (as might be expected), we find them employed upon Jupiter and his satellites, more than upon any other object. The phenomena of four moons were naturally interesting; nor was the inquiry unattended with important consequences. The velocity of light repaid the labours of Roemer, and the eclipses of the satellites opened a new, easy, and at that time a comparative accurate mode of determining the differences of longitude of distant stations. Hence astronomers were taught to look out for these phenomena, and their observations became recorded; in proportion, therefore, as the opportunities of observing these eclipses were more or less frequent, were they supplied with means of improving their tables; till at length something like accuracy was arrived at, as far as relates to those of the first and second satellites; to this also we must add another cause which will be found in the nature of the instruments at this time employed; for the rapid motion of these two satellites is such, that the intervening period between their first entrance into the shadow, and their complete obscuration by it, is short; hence telescopes were able to give something like uniformity to the observations of various observers—in short, theory and practice assisted each other.

But not so with the outer satellites; eclipses of them were comparatively of very rare occurrence, and the time of their entering the shadow till their complete obscuration being many times greater than in the case of the two first satellites, the observations became more difficult, and the instruments were inadequate to the purposes for which they were now wanted; observations, therefore, of different observers differed considerably with each other, and theory and practice were *everlastingly* at variance. Hence observations of these satellites came into disrepute, and almost into disuse.

At length, however, in the preface to a work entitled "*Tables Ecliptiques des Satellites de Jupiter*," the monstrous discordancies between the existing observations of the eclipses of the third and fourth satellites were dwelt upon, with *considerable energy*, by the celebrated Delambre; and, perhaps, to the sentiments expressed by this great man, may we trace the principal cause why at the present moment, observations of the eclipses of these two satellites are almost *generally* neglected. When, however, prejudice seems distributed by one whose name, like that of Delambre, is never mentioned but with respect, does it become dangerous, and more and more imperative is it, upon the humblest labourer in science, to point out the errors which it leads to—this must plead my excuse for the present communication.

As the work to which I allude, may not be in the library of every practical astronomer, I shall quote from it some of the passages calculated in my mind to prejudice observers.

Page 51.—Having alluded to some trifling equation which might be applied as a correction to his tables of the third satellite. He says, "Je n'ai poussé l'examen plus loin; mais il paraît en résulter que cette équation ne s'accorde pas plus avec l'observation qu'avec la théorie; et si la théorie est imparfaite, s'il reste à découvrir quelque inégalité sensible, elle dépend du moins d'un argument tout-à-fait différent. Ce n'est pas d'ailleurs sur quelques observations isolées qu'il faut chercher à rectifier des tables fondées sur 140 ans d'observations. A la vérité, toutes ces observations sont incertaines; mais celles qu'on leur opposerait ne sauraient être beaucoup plus sûres; et l'on en conviendra sans doute, quand on verra Messier et Méchain, dans la même ville et presque dans le même quartier, tous deux munis d'excellens instrumens et d'une vue excellente, ne s'accorder cependant qu'à quelques minutes près sur la même éclipse. Il est évident, de même, que pour les différences des méridiens, on ne peut se fier qu'au premier satellite; les autres ne sont guère bons qu'à éclaircir quelque point de physique céleste; le premier satellite est le seul qui puisse être vraiment utile aux astronomes et aux géographes. Mais ce satellite lui-même peut-il s'observer avec une précision bien parfaite? Quel astronome osera répondre de 10″ sur l'observation qui lui paraîtra faite dans les circonstances les plus favorables? En rassemblant les observations faites pendant cent ans à Paris et à Greenwich, on n'a pu trouver qu'à 10″ près la différence entre les deux méridiens."

Page 65.—He says, "D'après ces remarques, le calculateur ajoutera s'il juge à propos, 18 à 19″ à toutes nos époques du troisième satellite; mais nous sommes loin de lui garantir l'exactitude de cette correction; et que font en effet 18″ pour des observations qui ne sont presque jamais sûres à 2 ou 3 minutes?"

Page 49.—We have observations of immersion and emersion of the fourth satellite recorded,* and thus spoken of: "Pour cette dernière éclipse, les astronomes de Paris diffèrent entre eux de 3½′ sur chaque phase."

Page 85.—Speaking of eclipses of the fourth satellite, he says, "Je trouve entre les observations d'une même éclipse des différences qui vont à 7, 8, 10, 12, et 14′; et ce qu'il y a d'étonnant, c'est que ces discordances énormes ne sont pas toutes vers les limites. J'en trouve une qui est de 29′ 15″, mais la demi-durée calculée n'était que de 16′."

We are not told what instruments Messier and Mechain employed, whether immersions or emersions were observed; nor do we know any more, than that their observations on the *same* eclipse differed some minutes; hence therefore the statement only proves, that differences of longitude could not be gotten by reference to the computed tables.

The assertions that differences of meridians can only be procured by the first satellite; that *it* alone can be *truly useful* to astronomers and geographers; the confident tone in which our illustrious author asks, What astronomer will *dare* to answer to 10 seconds for the accuracy of an observation, which he shall

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* Im. observées.... 1h 37′ 41″ Em. observées.... 5h 7′ 7″
1 41 15 5 [illegible] 56

[illegible] made under the most favourable circumstances, cannot be made without making a deep impression on the mind of the reader. We must, however, remember, that these sentiments were founded upon comparison of all the observations of eclipses, which had been made from the year 1662 to the year 1802; and there will be little difficulty in believing that the chaotic mass would fully justify the assertion; for among the earlier observations immense discordancies would naturally be found; but I feel little hesitation in saying, that the observations which have been made *since* the year 1802 would be far more than sufficient to entitle the accuracy of the assertions, as far as they relate to modern observations, not only to be disputed, but even to be disproved. Observations made during 100 years at Greenwich and Paris, did not determine the difference of the two meridians nearer than 10 *seconds*. But observations made during seven months in Blackman-street and at Bushey, gave the difference of longitude to 17 hundredths of a second.

2. In a table which will be given presently, there are three observations of the third satellite, each coming far within two or three minutes of the truth; we shall also produce observations of eclipses of the fourth, liable to infinitely less error; hence I cannot coincide with our author, in the justice of his remarks.

3. From the recorded observations of immersions and emersions of the fourth satellite, it is evident enough that the incongruities are considerable; and if examined, as our own will hereafter be, they are found irreconcilable; hence supposing the same observers, the same instruments, and the same weather, at each station, both at the immersion and emersion, we may safely infer, that the telescopes were long and unmanageable;* and consequently would afford results *inconsistent* not only with *each other*, but with *themselves*.

4. As to the immense discordancies between the observations of the same eclipse related in the last paragraph; they do *indeed* place the observers and their instruments in the back ground; for it would be difficult to account why the observed periods of immersion or emersion should differ *more* than the time which the satellite requires to travel through a portion of space, equal to its own diameter.

On consideration of these passages then, I am induced to apply the sentiments they convey, more to the earlier observations, than to those of modern date; still there can be no doubt but that an immense quantity of the latter will be necessary, to invalidate the monstrous inaccuracies of the former. An observer, however, of the present day, will, I think, let his respect for the name of Delambre be great as it may, have some difficulty in supposing, that similar incongruities would attend observations of similar phenomena, *now* that telescopes are better made, and what is almost of as much importance, better *mounted*.

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* When it is known that they were from 15 to 20 feet in focal length, there will be some difficulty in arriving at any other conclusion.

That observations of eclipses of the fourth satellite are the most incongruous it would be silly to doubt, and unless the immersion and emersion can be procured, they cannot without much difficulty be brought to bear upon any useful point. The difference of the telescopes employed will alway need a correction; this may indeed be found by previous comparisons at the same stations; but as the *dissimilarity* of the weather at the places of observation will *materially* influence the results, when only the immersion or emersion be observed, an equation is wanting, not so *easily* to be found. Fortunately, however, the immersions and emersions of this, and the third satellite, are occasionally observable within a space of time little more than two hours; let us, therefore, inquire a little into *this* matter.

We have already noticed that the fourth satellite having entered the shadow, is a very considerable time before it becomes *lost* in it; hence its disappearance will be extremely gradual; let us suppose that during this time it passes through three distinct gradations of lustre; that at the first, it resembles the small star of ζ Ursæ Majoris; at the second, the small star of Polaris; and at the third, the small star of α Lyræ. Let us then have three telescopes; the one able only to show the small star of ζ; the second, only that of Polaris; the third adequate to show the small star of α Lyræ. On a fine night, all the telescopes show their respective objects very well—say at 10 o'clock; provided the stars have considerable altitude, and the weather be equally good, why should they not show them equally as well at 12 o'clock? It would puzzle most persons, I think, to determine; if so, provided the analogy hold good, why may not the evanescence, and re-appearance of the fourth satellite be observed, within reasonable limits? I confess I see no reason. I know it is said, an observer will have an impression left on his mind, that the satellite continues visible when it *really* is not so; but this is not distinctly proved; and again on the emersion, knowing the point at which he is to look for it, he thinks he sees it earlier than he actually does; this again is not proved: we will, however, allow them both, and we shall be as if they were not allowed at all. The only difference between the three telescopes would be, that A would give the immersion *earlier* than B; and B *sooner* than C. And at the emersion, A would show the satellite *later* than B, and B *later* than C; circumstances, as we shall hereafter see, not of the *least* importance.

Entertaining then sentiments such as these, I determined on the first favourable opportunity, to observe the immersion and emersion of the fourth satellite, with every *possible care*; and the first day of the present month enabled me to do so. Jupiter's meridian altitude was about 62°; the immersion occurred when the planet was $1^h\ 34'$ east of the meridian; the emersion when it was 34 minutes west of it. The memoranda relative to the observations, as entered in the Journal at the time are as follow:

"Prior to taking the transit of Aldebaran, the five feet equatorial was placed upon Jupiter, and immediately after the transit had been secured, I went to see how matters were going on; the fourth satellite was at this time about the splendour of a star of the ninth magnitude, and of a light blue colour. The night was remarkably clear; I determined to remain at the instrument as long as the satellite continued visible, which, according to the Nautical, would be at least a quarter of an hour. To make myself comfortable, therefore, I placed some blocks of wood upon the double steps, and took my seat very quietly: the lights, except that at the clock, were all put out; it continued diminishing in lustre till 4h 34′ 57″ per clock, at which time I could see it no more. I observed it of the brightness of the small star near α Lyræ for more than a minute.

"Emersion of the fourth satellite (observatory darkened as before) at 6h 43′ 5″ per clock.

"At 6h 45′ about as bright as the pole star.

"At 6h 52′ had not half the splendour of the dullest of the other three which were visible. Unable to spare more time, further observation was given up."

Greatly satisfied with my own observations, I was in hopes that the extraordinary fineness of the night would have rendered correspondent ones, at various stations, almost certain; but in this I have had the mortification to find myself disappointed. The only observer who, as far as I have ascertained (and I have made very extensive inquiry), was similarly engaged with myself, is to be found in Col. Beaufoy: it is however indeed fortunate that it was he. My opinion of his observations of the eclipses of Jupiter's satellites, has long been before the public; so that I cannot now be suspected of commending his accuracy, merely to suit my present purpose.

My observations made with the five feet equatorial; the object glass of its telescope has 64 inches focus, and 3¾ inches clear aperture; power 133.

Col. Beaufoy's telescope has an object glass, precisely of the same diameter, but of 56·5 inches focal length: it is mounted on a *very steady* stand, but not *equatorially*. Magnifying power used = 86.

Prior to observation, he excluded all light from his observatory, except what was sufficient to enable him to distinguish the bands of his clock.

Longitude of Blackman-street observatory . . . = 0′ 21·76″ W.
Bushey . . . . = 1 20·93 W.
Diff. of long. Bushey to the W. . . . . = 0 59·17

Blackman-street Observations.				Bushey Observations.			
Immersion	4h	35′	3·80″	Immersion	4h	32′	27·09″
Emersion	6	48	11·89	Emersion	6	43	50·46

(Sidereal time at each station.)

Now if we add the difference of longitude to the Bushey observation of immersion, we shall have in *Blackman-street time*, the instant at which the phenomenon was observed at Bushey; and the difference, if any, between this and the Blackman-street observation, will show the time by which the satellite was seen longer, at one station than at the other.

4h 32′ 27·09″ = Immersion at Bushey.
+ 59·17 = Difference of longitude.

4 33 26·26 = Blackman-street time when the immersion was observed at Bushey.
4 35 3·89 = Immersion at Blackman-street.

+1 37·63 = the time that the satellite was seen *longer* at Blackman-street than at Bushey.

A mere inspection of the times shows, that the differences are *considerable*: let us see if they are *reconcileable*. As the telescopes at the two stations are nearly similar; as the same precaution of excluding adventitious light was employed at both; we must seek for the cause of the discrepancy, either in a *clearer* atmosphere at one observatory than at the other—in a *greater* sensibility to minute particles of light, which one observer has than the other—or in the *superior* steadiness of the one instrument over the other. As to difference of atmosphere, we have no proof that there was any; indeed the probability is in favour of Bushey; *it* being situated *far* from any frequented neighbourhood; while the Blackman-street observatory is surrounded by buildings in *every direction*. As to increased sensibility to small particles of light, we have no good grounds to suspect that one observer possesses this, *more* than the other. Hence we are left to the only remaining source of discrepancy, namely, the greater steadiness which one instrument has than the other; and this there can be no doubt the Blackman-street instrument possesses: it can be moved in right ascension by the finger and thumb; a star may be kept bisected by one of its micrometer wires any reasonable time; nor will any tremulous motion be communicated to the star, although a power of 500 or 600 be employed. Hence the experience of daily observation would authorize us to declare, *cæteris paribus*, that in Blackman-street the immersion ought to be seen *later* than at Bushey: perhaps also the trifling difference of focal length and magnifying power of the Blackman-street instrument may contribute some little to the result; which is, that the immersion was seen *later* in Blackman-street than at Bushey by 1′ 37·63.″

But if our reasoning be correct, the emersion should be seen earlier in Blackman-street; and if the weather at the emersion be the same at each station, as it was at the time of immersion, by as much as the immersion was seen later in Blackman-street, should the emersion be seen earlier; a little allowance being made for error of observation: let us see if it were so,

6h 43′ 50·46″ = Emersion at Bushey.
+ 59·17 = Difference of longitude.

6 44 49·63 = Blackman-street time when the emersion was observed at Bushey.
6 43 11·89 = Emersion at Blackman-street.

−1 37·74 = the time that the satellite was seen *earlier* at Blackman-street than at Bushey.

Hence it appears that the emersion was seen *earlier* in Blackman-street than at Bushey; and by an interval of time agreeing with that by which the immersion was observed *later*, to 11 hundredths of a second.

Let us now see how far the results can be converted to *practical* utility—to what degree of accuracy then, will they enable us to determine the difference between the meridians of the two observatories.

Im. at Blackman-street = 4h 35′ 3·89″
Bushey....... = 4 32 27·09

Hence diff. of longitude = 2 36·80 Bushey to the W.

Em. at Blackman-street = 6h 43′ 11·89″
Bushey....... = 6 43 50·46

Hence diff. of longitude = 38·57 Bushey to the E.

Now + 2′ 36·80″ or W.
− 0 38·57 or E.

2) + 1 58·23 W.

Difference of longitude .... = 0 59·11 Bushey being to W.
But known difference ..... = 0 59·17

Error of observation .... = 0 0·06 at the two Stations.

Consequently the difference of longitude, between the two observatories of Blackman-street and Bushey, is ascertained to *six-hundredths* of a second; a quantity therefore which we must consider, as the *error* of observation at the *two* stations.

But some time since, the difference of the two meridians was found by the same observers, with the same instruments, and the results will be shown in the following table:—

Difference of Longitude of my Observatory and Col. Beaufoy's, by Observations of 14 Eclipses of Jupiter's Satellites, made at our respective Stations.

		My Observations.			Col. Beaufoy's Observations.			Difference of Longitude.	
1821.									
Aug. 4	Imm. 2 Sat.	11h	5′	26·80″	11h	4′	31″	0′	55·80″ W.
18	Imm. 1	12	0	8·09	11	58	53	1	15·09
Oct. 24	Em. 3	10	35	8·40	10	34	22	0	46·40
28	Imm. 1	9	10	40·76	9	9	51	0	49·76
Nov. 4	Em. 1	11	6	40·83	11	5	19	1	21·83
20	Em. 1	9	25	26·26	9	24	34	0	52·26
27	Em. 1	11	21	4·56	11	20	19	0	45·56
29	Em. 1	5	49	53·60	5	49	13	0	40·60
29	Em. 3	6	42	49·03	6	42	8	0	41·03
Dec. 6	Em. 1	7	45	55·10	7	45	20	0	35·10
1822.									
Jan. 14	Em. 1	6	23	40·48		22	27	1	13·48
29	Em. 2	6	55	45·30	6	54	15	1	30·30
Feb. 23	Em. 3	7	8	26·90	7	7	13	1	13·90
March 1	Em. 1	6	57	36·60	6	56	27	1	9·60

Mean differ. of long. of the two Observatories . . = 0 59·34
Known difference of the two Observatories . . . . = 0 59·17
Error of observation at the two Stations . . . . . . = 0 0·17

Thus it seems that 14 observations, nine of the first satellite, two of the second, and three of the third, gave the difference of longitude to seventeen-hundredths of a second, and that it was the work of *seven months*; while in the observation before narrated, the accuracy is *three times* as great, and is obtained in a few minutes more than *two hours*. Some perhaps will contend, that this accuracy is the offspring of accident; it is however at *least* as probable, that accident has had nothing at all to do with it; on the same *supposition*, and with equal plausibility, might it be urged that accident has prevented accuracy in each of the 14 observations, principally of the *favourite* satellite; the two nearest of which to accuracy, are 50 or 100 times more remote from it, than are the results of those observations, which are the immediate objects of this communication.

From this however let it not be supposed, that I imagine equal accuracy will always be procured; an observer *may* and occasionally *does* obtain the right ascension of a star by one wire of his transit instrument just as correctly as if he employ the five or the seven; yet he must not suppose he will always do it; just so with the observations of immersion and emersion I have alluded to; all I contend for is, provided the same care—the same instrument—the same magnifying power—the same precaution—the same observer—the same weather—be used, found, or employed, at the various stations both at the immersion, and subsequent emersion, that the results so far from meriting obloquy, will probably be far more *accurate* than any two eclipses of either the first or second satellites—and if what be stated prove true of the fourth satellite, it cannot be otherwise of the third.

But in the instance we have dwelt upon, only two observers were concerned; further observations therefore should be recurred to—those then whose stations relatively to Greenwich are well settled—whose instruments are well mounted, and whose time is well known, will do an essential service to practical astronomy, by observing the immersions and subsequent emersions of the third and fourth satellites whenever they are visible—they should be at their telescopes some eight or ten minutes before the phenomena are expected; should take every possible care that the immersion and emersion be observed under similar circumstances—and should note at the time, how far the weather at the emersion coincided with that in which the immersion was observed; nor will the relative splendour of such other satellites, as may be visible at the two periods, allow an accustomed eye much difficulty in deciding.

I have dwelt upon this subject perhaps longer than some will say, its importance warrants—two marked instances of the injurious effects of the opinions generally entertained against observations of the fourth satellite have however recently presented themselves—One individual many years an accurate and assiduous observer assures me, that in consequence of prejudice, he never looked out for an eclipse of the fourth satellite in his life.—And another astronomer did not observe the very eclipse here so often referred to, "because of sentiments he entertained founded on Delambre's statements."—Nor are the individuals referred to of any mean importance—the latter is well known as the author of many *useful* astronomical publications; whilst to the former, practical astronomy owes *greater obligations*, than to any *person in existence*.

To remind observers a table is subjoined giving in sidereal and mean time the predicted immersions and emersions of the third and fourth satellites during the next two months, and should the weather prove favourable, I have little doubt the result will show; that the opinions generally entertained of the *inutility* of observations of eclipses of the Third and Fourth satellites, *originate* in PREJUDICE, and *terminate* in ERROR.

		Sidereal Time.	Mean Time.
March 2.	Immersion third satellite ..	8h 0′	9h 18′
	Emersion	11 13	12 30
April 8.	Immersion fourth satellite ..	9 13	8 4
	Emersion	12 6	10 58

It is almost needless to say that *where* the satellite is lost at the immersion, it may be looked for at the emersion.

James South.

Article XIV.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1823. Part II.

(*Continued from p. 147.*)

XXI. *Second Part of the paper on the Nerves of the Orbit.* By Charles Bell, Esq. Communicated by Sir Humphry Davy, Bart. Pres. RS.

The following extract from the concluding pages of this paper, gives the general results of Mr. Bell's investigation of the nerves of the head.

"I hope I have now unravelled the intricacy of the nerves of the head, and have correctly assigned to each nerve its proper office. In our books of Anatomy, the nerves are numbered according to the method of Willis, an arrangement which was made in ignorance of the distinct functions of the nerves, and merely in correspondence with the order of succession in which they appear on dissection.

"The first nerve is provided with a sensibility to effluvia, and is properly called olfactory nerve.

"The second is the optic nerve, and all impressions upon it excite only sensations of light.

"The third nerve goes to the muscles of the eye solely, and is a voluntary nerve by which the eye is directed to objects.

"The fourth nerve performs the insensible traversing motions of the eyeball. It combines the motions of the eyeball and eyelids, and connects the eye with the respiratory system.

"The fifth is the universal nerve of sensation to the head and face, to the skin, to the surfaces of the eye, the cavities of the nose, the mouth and tongue.*

"The sixth nerve is a muscular and voluntary nerve of the eye.

"The seventh is the auditory nerve, and the division of it, called *portio dura,* is the motor nerve of the face and eyelids; and the respiratory nerve, and that on which the expression of the face depends.

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[*] "In this view of the fifth nerve, I have not touched upon its resemblance to the spinal nerves. But if we had ascended from the consideration of the spinal nerves to the nerves of the head, we should then have seen that the fifth was the spinal nerve of the head; that it had a ganglion at its root, a double origin, and from its power over the muscles of the jaws and mastication, that it was a double nerve in function, being that nerve which bestows sensibility, at the same time that it sends branches to the original muscles; that is to say, to that class of muscles which are common to animals in every gradation. In all these respects it resembles the spinal nerves."

"The eighth, and the Accessory nerve, are respiratory nerves.

"The ninth nerve is the motor of the tongue.

"The tenth is the first of the spinal nerves; it has a double root and a double office; it is both a muscular and a sensitive nerve.

"Had I taken the nerves of any other complex organ rather than of the eye, I should have had an easier task. If I had taken the nerves of the tongue, I should have been able to prove by experiment, and in a manner the most direct, that the three nerves belong to three distinct functions, and stand related to three different classes of parts. I could have shown that taste and sensibility belong to the office of the fifth nerve, voluntary motion to the ninth, and deglutition to the glossopharyngeal nerve of the tongue."

XXII. *An Account of Experiments made with an Invariable Pendulum at New South Wales* by Major-General Sir Thomas Brisbane, KCB. FRS. Communicated by Capt. Henry Kater, FRS. in a Letter to Sir Humphry Davy.

The following are the results of these experiments, as given by Capt. Kater:—

"If the number of vibrations resulting from Sir Thomas Brisbane's experiments at Paramatta be compared with the mean number of vibrations made by the pendulum at London, we shall have 39·07696 inches for the length of the pendulum vibrating seconds at Paramatta; ·0052704 for the diminution of gravity from the pole to the equator; and $\frac{1}{295\cdot84}$ for the resulting compression; the length of the pendulum vibrating seconds at London being taken at 39·13929 inches.

"The experiments at Paramatta being compared with those made by me at Unst, in latitude 60° 45′ 28″ north, give ·0053605 for the diminution of gravity from the pole to the equator, and $\frac{1}{303\cdot95}$ for the resulting compression.

"If Mr. Dunlop's experiments at Paramatta be compared with those made at London, we obtain 39·07751 for the length of the seconds' pendulum at Paramatta, ·0052238 for the diminution of gravity from the pole to the equator, and $\frac{1}{291\cdot83}$ for the compression. Or, comparing Mr. Dunlop's experiments with those made at Unst, we have ·0053292 for the diminution of gravity from the pole to the equator, and $\frac{1}{301\cdot09}$ for the resulting compression.

"The compressions here deduced must not as yet be deemed conclusive; for it is well known that a very small alteration in the number of vibrations made by the pendulum would occasion a considerable difference in the fraction indicating the compression. The indefatigable zeal of Sir Thomas Brisbane will, however, no doubt soon furnish additional data."

"P.S. I may here take the opportunity of correcting an error in the "Account of Experiments for determining the Variation in the Length of the Pendulum vibrating Seconds at the principal Stations of the Trigonometrical Survey of Great Britain."

"In the first series of observations made with the repeating circle for the latitude of Clifton, 1′ 41·6″ has been applied as the correction for the level instead of 141·6″ = 2′ 21·6″. The resulting latitude, when the proper correction is made, is 53° 27′ 44·94″ instead of 53° 27′ 40·94″, and the greatest difference between the five independent latitudes of Clifton 3·48″ instead of 5·24″."

XXIII. *On the Daily Variation of the Horizontal and Dipping Needles.* By Peter Barlow, Esq. FRS.

An abstract of this paper will be found in the present number of the *Annals*, at p. 163.

XXIV. *On the Diurnal Deviations of the Horizontal Needle when under the Influence of Magnets.* By Samuel Hunter Christie, Esq. MA. Fellow of the Cambridge Philosophical Society: of the Royal Military Academy. Communicated by Sir Humphry Davy.

Our present limits will not permit us to give any account of this extended paper, occupying 50 pages; but we shall probably devote a separate article to that purpose.

XXV. *On Fossil Shells.* By Lewis Weston Dillwyn, Esq. FRS. In a Letter to Sir H. Davy.

This paper we have reprinted entire at p. 177

(To be continued.)

Article XV.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Jan. 15.—Messrs. J. H. Vivian, and Michael Faraday, were respectively admitted Fellows of the Society; and the reading of Messrs. Herschel and South's "Observations on the Positions and Distances of Three Hundred and Eighty Double and Triple Fixed Stars," was resumed and concluded.

Jan. 22.—Dr. C. Scudamore was admitted a Fellow of the Society; and the following paper was read:

"On a Mode of preventing the Corrosion of Copper-Sheathing, by Sea-Water, in Ships of War, and other Ships." By Sir Humphry Davy, Bart. PRS.

The attention of the President having been drawn to this subject by the Commissioners of the Navy Board, he instituted a series of experiments upon it, and has discovered a simple and effectual mode of remedying the evil. Copper, when immersed in sea-water, however pure and malleable it may be, becomes covered with a coat of a green submuriate, a sort of rust, which, when washed off, is succeeded by a similar one, and the process continues until the metal is completely destroyed.

It was evident that no alteration which could be effected in the copper would prevent its corrosion; the effect on different kinds of copper might be somewhat different, but the principal diversities must be owing to the variations in the saltness and temperature of the sea-water.

Sir Humphry was led to the discovery, by the same principle which led him to that of the decomposition of the alkalies; namely, that chemical affinities might be balanced or destroyed, by changing the electrical states of the substances: it thence appeared that the corrosion of the copper might be prevented by its being brought, by contact with another metal, into a negatively electric state; and he had accordingly found that by the contact of tin, forming part of an electrical circuit, of $\frac{1}{100}$th part the surface of the copper, the desired effect was completely obtained. Other metals, positive in respect to copper, may be employed, as lead and zinc, but tin is preferable, on account of its capability of being brought into complete contact with the copper, by means of solder, and also because its submuriate is easily detached from the metal.

The experiments were made with ribbands of tin, and it was found that such a ribband, equal in substance to only $\frac{1}{200}$th part of the copper, effectually prevented the corrosion of the latter. They were so entirely satisfactory, that not the smallest doubt can be entertained of the perfect success of the method in practice; and the Lords Commissioners of the Admiralty have made arrangements for enabling the President to repeat them on the largest scale, on ships of war.

It is probable, Sir Humphry observes, that this method, besides preventing oxidation, will also prevent the adherence of vegetables and marine animals to the sheathing.

This interesting communication terminated with some allusions to the great importance of the discovery it announced, in a national point of view, with respect to our maritime and commercial interests.

The reading was likewise commenced of a paper, "On the Development of Magnetical Properties in Iron and Steel by Percussion, Part II." By W. Scoresby, Jun. FRSE. Communicated by Sir H. Davy.

Jan. 29.—Thomas Amyot, Esq. VPSA. was admitted a Fellow of the Society, and the reading of Mr. Scoresby's paper was terminated.

This communication was a continuation of a former paper by Mr. Scoresby, under the same title, which appeared in the Phil.

Trans. for 1822. (See *Annals*, N. S. v.) In the first part, Mr. S. describes his new process for the development of magnetism, and gives the result of a number of experiments made with different kinds of iron, and under different modes of treatment. The only experiments at all analogous to these were performed by Dr. Gilbert about two centuries back, in which Dr. G., hammering a piece of iron in the direction of the magnetic meridian, and drawing it out while red-hot, gave it such a degree of magnetism as to cause it, when floated by a piece of cork on water, to adjust itself in a north and south direction. But Dr. Gilbert went no further. Mr. Scoresby, however, considering, that as magnetism in steel is more readily developed by the contact of magnetizable substances, and particularly if these substances be already magnetic, imagined, "that the magnetizing effects of percussion might be greatly increased by hammering a steel bar with its lower end resting upon the upper end of a large rod of iron or soft steel, both the masses being held in a vertical position; and that if the rod were first rendered magnetic by hammering, the effect on the steel bar would probably be augmented." The experiments instituted to ascertain the effect of such treatment fully proved that these opinions were correct. A small bar of soft steel being hammered while resting upon a surface of stone or metal, not ferruginous, was rendered capable of lifting 6½ grains of iron, which was the extreme effect; but on being hammered while held vertically upon a parlour poker, also held erect, it lifted a nail of 88 grains weight after 22 blows.

The paper now communicated to the Royal Society described a new arrangement and process, by which a much higher degree of magnetic energy was developed. In the former experiments of Mr. Scoresby, a single rod of iron only was used, and the steel bars or wires were hammered upon it, while both were held in a vertical position; in which case the magnetism of the iron, after hammering, was employed in aid of the power of percussion for the development of the magnetism of the steel bars. But the iron acted only on the lower end of the steel wires; the magnetism of the upper end being spontaneous, or what is by magneticians called *consequential*. Hence, Mr. S. attempted to supply an additional force for the development of the magnetism of the steel, to act upon the upper end of the wire as well as on the lower, and this he accomplished by hammering the wire or bar of steel between two bars of iron. The bars of iron he used were three feet and one foot in length, both made of common iron. The steel consisted of wires of about one-eighth of an inch in diameter. The lifting power produced in the wires was estimated by the heaviest of a series of nails, polished at the points, which the wire was capable of lifting.

We cannot follow Mr. Scoresby through the details of his experiments; but we may state a few particulars of the results which he obtained from his investigations.

1. By Mr. Scoresby's first process (which he denominates the *simple process*, to distinguish it from the second, or *compound process*), he obtained a maximum magnetical effect on a steel wire of about six inches long, capable of lifting a nail of 186 grs. which effect the compound process raised up to 326 grs. In other cases, an equal and sometimes a superior effect was produced.

2. In respect of *temper* or degree of hardness of the wires, it was found that the softest wires obtained generally the highest power, and were most easily magnetized, but the effect soon went off.

3. By using a larger bar of iron (about eight feet in length), a great increase of magnetical power was obtained, a wire of only six inches long being made to lift, by hammering by the compound process on this bar, a weight of 669 grains, or *four* times the weight of the wire.

4. The limit to the magnetism given to the wires, Mr. Scoresby considers to be determined by the magnetism of the iron bars employed. The bars being simply placed vertically, become slightly magnetic by *position* from the earth. This polarity is increased by hammering them while they remain in a perpendicular position. An increase of magnetism continues to obtain by repeated hammering the bars up to the extent that Mr. Scoresby developed. But the *maximum* required the bars and wires to be very often hammered, and the process to be continued at intervals for a few minutes at a time, during several days. For a wire, however, to be made to lift its own weight required only a few minutes hammering, and when the bars had become magnetic by use, a single blow with a hammer was sometimes found sufficient to enable the wire to lift its own weight. To produce the best effect, it is important to have the steel wires polished at the end, and always to use the same end *downward*, which obtains north polarity; for by this means, Mr. S. found that an increase of capacity for magnetism in the wires took place after almost any operation.

Mr. S. conceives that the high effect obtained by percussion depends on the disposition that percussion gives to the ferruginous particles, for assuming that condition to which we apply the name magnetic. The particles of ferruginous substances, especially steel, resist this condition to a certain extent, which resistance percussion tends to overcome. The general law Mr. S. resolves into this; *that percussion on magnetizable substances in mutual contact inclines them to an equality of condition.* And this effect he illustrates, by the tendency of bodies unequally heated, to assume, when placed together, the same temperature. And from the tendency of the cooler bodies to acquire temperature, and the hotter to lose temperature, he explains the apparently opposite proposition, that magnetism is both developed and destroyed by percussion. The power of strong magnets is diminished by hammering, if held in the air unsupported, or rested upon any body not equally magnetic; and the power of very weak magnets, or bars with little or no magnetism, is increased, if held upon any substance that is magnetic. In all cases and circumstances, the hammering tends to bring the substances in contact into a similar state, the weaker being strengthened, and the stronger weakened.

A paper was also read, entitled "Observations on the *Iguana tuberculata*, the common Guana." By the Rev. Lansdown Guilding, BA. FLS. Communicated by Sir E. Home, Bart. VPRS.

This paper was very short: it commenced with some general remarks on the necessity, in zoology, of describing animals from living specimens, and on the errors which had been committed by naturalists in stating the characters of certain lizards, in consequence of inattention to that circumstance; thus the gular process of the lizards alluded to, had been erroneously described as a pouch capable of dilatation. Mr. Guilding then proceeded to describe briefly an organ on the parietal bones of the head of the guana, to which he gave the name of *foramen Homianum*, in honour of Sir E. Home.

Feb. 5.—A paper was communicated, entitled, "A finite and exact Expression for the Refraction of an Atmosphere nearly resembling that of the Earth." By Thomas Young, MD. For. Sec. RS.

The reading was commenced of the Bakerian Lecture, by J. F. W. Herschel, Esq. FRS.

Feb. 12.—The Bakerian Lecture was concluded.

The subject of this Lecture is the phenomena exhibited by mercury, and other fluid metals, when placed within the influence of an electric current transmitted through conducting liquids.

If a quantity of mercury be placed in a dish and covered with a conducting liquid, through which an electric current is transmitted from a voltaic pile of moderate energy, by wires not in contact with the mercury, this metal will be thrown into a state of circulation, the force and direction of which varies with the nature of the liquid, the intensity of the electric power used, and other adventitious circumstances. If the liquid be sulphuric, phosphoric, or any of the more concentrated acids, the circulation is excessively violent, even with weak electric powers, and takes place in a direction *from* the negative to the positive wire. On the other hand, under alkaline solutions, *pure* mercury remains at perfect rest in like circumstances; but if the least atom of potassium, sodium, zinc, or any metal more electro-positive than mercury, be added to it, a violent rotation is immediately produced, in the opposite direction, or from the *positive* wire. From some trials, Mr. Herschel is led to conclude, that much less than a millionth part of potassium, or a 100,000th of zinc, is sufficient to impart this singular property to mercury. Lead and tin act with much less energy. Bismuth, copper, silver, and gold, not at all. A number of singular phænomena in the electrization of mercury and other metals are described; and some calculations added respecting the intensity of the forces acting on the molecules of the electrified body, which Mr. H. concludes, in his experiments, to have been not less than 50,000 times their gravity.

In the sequel, Mr. Herschel notices the curious gyratory motions, observed by M. Serrulas, in fragments of alloy of potassium and bismuth, when floated on mercury under water; the cause of which he shows to have been misunderstood by Mr. S. and which admit of easy explanation on the principles of this Lecture.

For the sake of such of our readers as may wish to repeat these experiments, we may mention, that it is absolutely necessary to use mercury recently distilled and purified, by washing with dilute nitric acid, and that all the vessels employed must be scrupulously clean, and the surface of the metal free from any adhering film. A small battery of eight or ten pairs of single plates is sufficient to exhibit all the phænomena.

Article XVI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Primary Forms of Sulphur.*

Our readers may have remarked in the present volume of the *Annals*, a description by Mr. Brooke, of two primary forms of sulphate of nickel. A paper appears in the Annales de Chimie for Nov. 1823, by Mr. Mitscherlich, announcing the discovery of two primary forms of sulphur. The one, that which occurs in nature, *an octahedron with a rhombic base;* the other, *an oblique rhombic prism*, P. on M. measuring 94° 5′ and M. on M. 90° 32′, produced by fusing sulphur in a vessel, in which it may very gradually cool; when a crust is formed round the mass, it is to be broken, and the sulphur which remains fluid to be poured out; the crystals will appear lining the cavity.

Mr. M. appears to consider that all substances which produce crystals, may likewise assume two primary forms.

This gentleman has crystallized phosphorus from a solution of phosphuret of sulphur, the form of which is *the rhombic dodecahedron.*

II. *Uranite of Autun.*

M. Laugier has submitted the uranite of Autun to a fresh examination; the results are:

Water	21·0
Oxide of uranium	55·0
Phosphoric acid	14·5
Lime	4·6
Oxide of iron and silica	3·0
Traces of manganese and tin.	
	98·1

From this analysis M. Laugier draws the following conclusions:

1st, That the uranite of Autun, hitherto considered either as an oxide of uranium, or as a compound of the oxide of lime, is a true phosphate of uranium.

2dly, That the lime in this mineral, is for the most part, in an uncombined state.

3dly, That phosphate of uranium is entirely soluble in carbonate of ammonia, from which it is totally precipitated by ebullition.—(Annales de Chimie et de Physique, vol. xxiv. p. 247. Nov. 1823.)

The existence of phosphoric acid in the ore of uranium was announced by me, in the *Annals* for December 1822. I then supposed, though erroneously, that the fact was new, but it had been entirely overlooked. In the *Annals* for January 1823 (p. 61), I stated, precisely the same opinion of the real nature of the uranite of Autun, as M. Laugier has announced, that it is "essentially composed of phosphate of uranium."

M. Laugier fairly acknowledges that he had heard of the fact of the phosphoric acid having been noticed in England, but only since the reading of this paper on the 15th of September last.—*Edit.*

III. *Phosphorescence of Acetate of Lime.*

(To the Editor of the *Annals of Philosophy.*)

SIR, *Jan.* 14, 1824.

Not being aware that the phosphorescence of this salt has been noticed by any chemical writer, I have taken the liberty to forward you the observations I have made on its peculiar properties in this respect. Dissolve any quantity of acetate of lime in water, and place it on a sand heat, in a wedgwood ware dish, evaporate to dryness without disturbing it. When quite dry, let the bulb of a thermometer be rested on the bottom of the dish, and when the temperature has attained the 250th of Faht, the lime will be found to adhere very firmly. If light be now excluded, and the acetate strongly rubbed with a stiff spatula, it will become highly luminous. The high temperature required for producing this appearance is peculiar to this substance and fluate of lime.

I am, Sir,
Your obedient servant,
NICHOLAS MILLS.

IV. *Chemical Examination of a Fragment of a Meteor which fell in Maine, August, 1823.* By J. W. Webster, MD. MGS. Lond. &c.

This aërolite fell at Nobleborough in the State of Maine, on the 7th of August, 1823, between four and five o'clock P.M. The only information which I have been able to obtain of the attending phænomena is from the papers of the day, and from a communication of Professor Cleaveland, which is published in the American Journal of Science, vol. vii. p. 170; this account he informs me was obtained at his request by a gentleman of intelligence in a personal interview with Mr. A. Dinsmore, who was at work near the place where the aërolite struck. "Mr. Dinsmore's attention was excited by hearing a noise which at first resembled the discharges of platoons of soldiers, but became more rapid in succession. The air was perfectly calm; and the sky was clear, with the exception of a small whitish cloud, apparently about forty feet square, nearly in his zenith, from which the noise seemed to proceed. After the explosion, this little cloud appeared to be in rapid spiral motion downwards, as if about to fall on him, and made a noise like a whirlwind among leaves. At this moment, the stone fell among some sheep, which were thereby much frightened, jumped, and ran into the woods. This circumstance assisted Mr. D. in finding the spot where the stone struck, which was about forty paces in front of the place where he was standing. The aërolite penetrated the earth about six inches, and there meeting another stone, was broken into fragments. When first taken up, which was about one hour after its fall, it exhaled a strong sulphureous odour. The whole mass previous to its fracture probably weighed between four and six pounds; other fragments of the same meteoric stone are said to have been found several miles distant from Nobleborough."—*Amer. Jour.*

To the politeness of Dr. George Hayward I am indebted for a fragment of this meteor.

Externally the specimen was in part covered with a thin semi-vitrified crust or enamel of a black colour, the surface of which was irregular and marked with numerous depressions, presenting every appearance of having been subjected to intense heat. The crust was hard, yielding with difficulty to the knife. The quantity of this crust which the small fragment I obtained afforded, was not sufficient to allow of any separate analysis of it.

The mass of the specimen had a light gray colour interspersed with oblong spots of white, having the aspect of decomposed leucite, and giving it a porphyritic aspect. Throughout the stone minute points of a yellow substance, resembling olivine, were distributed, with microscopic points of a yellow colour, which I imagine were sulphuretted iron. The cement by which these substances were united was of an earthy aspect, and soft texture, readily broken down by the fingers. The general appearance of the mass was precisely like that of some of the volcanic tuffas.

The specific gravity was remarkably low, being but 2·05.

Before the blow-pipe it exhaled a sulphureous odour, but was not fused.

The specimen was reduced to powder and submitted to the action of a magnet of considerable power, but no attractable particles were separated. A portion was heated to redness on a platina spoon; it emitted the sulphureous odour, and its weight was diminished rather more than 21 per cent.; the residue acquired a brown colour; it was again presented to the magnet, but nothing was attracted.

The composition of this meteoric mass I found to be:

Sulphur	18·3
Silex	29·5
Alumina	4·7
Lime	a trace
Magnesia	24·8
Chrome	4·0
Iron	14.9
Nickel	2·3
	98·5
Loss	1·5
	100·0

(Phil. Mag. lxiii. 16—19.)

Article XVII.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

C. Tennant, Esq. has in the press, in 2 vols. 8vo. a Narrative of a Tour through Parts of the Netherlands, Holland, Germany, Switzerland, Savoy, and France, in the Years 1821, 1822, including a Description of the Rhine Voyage in the middle of Autumn, and the Stupendous Scenery of the Alps in the depth of Winter.

Shortly will be published, by Mr. Benecke, of Lloyd's, a Treatise on the Principles of Indemnity in Marine Insurance, Bottomry, and Respondentia, containing practical Rules for effecting Insurances, and for the Adjustment of all Kinds of Losses and Averages.

Capt. Parry's Second Voyage of Discovery is nearly ready for publication.

Thomas Hewson, AB. is about to publish Observations on the History and Treatment of the Ophthalmia accompanying the Secondary Forms of Lues Venerea.

Moscologia Britannica, containing the Mosses of Great Britain and Ireland, systematically arranged and described; by W. J. Hooker, and Thomas Taylor, MP. is in the press.

Mr. George Dyer is preparing "The Privileges of the University of Cambridge."

JUST PUBLISHED.

A Selection of the Geological Memoirs contained in the Annales des Mines, with a Synoptical Table of Equivalent Formations, in English, French, and German, &c. With 11 Plates. 8vo. 18s.

A Grammar of Infinite Forms; or the Mathematical Elements of Ancient Philosophy and Mythology; by W. Howison. Post 8vo. 5*s.*

A Compendious View of the History of the Darker Ages; by C. Chatfield, Esq. 8vo. 7*s.* 6*d.*

Prodromus Systematis Universalis Regni Vegetabilis, auctore Aug. P. de Candolle. Part I. Thick 8vo. 1*l.* 5*s.*

Article XVIII.

NEW PATENTS.

G. M. Glascot, Great Garden-street, Whitechapel, brass-founder, and T. Michell, Upper Thames-street, gent. for their improvements in the construction or form of nails to be used in or for the securing of copper and other sheathing on ships, and for other purposes.—Dec. 9.

T. Horne, the younger, Birmingham, brass-founder, for improvements in the manufacture of rack pullies in brass or other metals.—Dec. 9.

W. Furnival, Droitwich, salt-manufacturer, and A. Smith, Glasgow, master-mariner, for their improved boiler for steam-engines and other purposes.—Dec. 9.

Sir H. Heathcote, Surry-street, Strand, for his improvement of the stay-sails generally in use for the purpose of intercepting wind between the square sails of ships and other square-rigged vessels.—Dec. 13.

J. Boot, Nottingham, lace-manufacturer, for his improved apparatus to be used in the process of singing lace and other purposes.—Dec. 16.

P. J. B. V. Gosset, Queen-street, Haymarket, merchant, for producing various shapes, patterns, and sizes from metals, or other materials, capable of receiving an oval, round, or other form.—Dec. 18.

T. Greenwood, Gildersoun, near Leeds, machine-maker, and J. Thackrah, surgical mechanist, Leeds, for their improvements in patterns and clocks.—Dec. 27.

J. Vallance, Esq. Brighton, for his improved methods of freezing water.—Jan. 1.

F. Devereux, merchant, Cheapside, for certain improvements on the mill or machine for grinding wheat and other articles, commonly known by the name of the French Military Mill.—Jan. 8.

J. Foot, Charles-street, Spitalfields, silk-manufacturer, for his improved umbrella.—Jan. 15.

J. White, New-road, Marylebone, architect, for his floating breakwater.—Jan. 15.

J. Finlayson, Muirkirk, Ayrshire, farmer, for certain improvements on ploughs and harrows.—Jan. 15.

Jean le Grand, Leman-street, Goodman's-fields, vinegar-manufacturer, for certain improvements in fermented liquors, and the various products to be obtained therefrom.—Jan. 15.

W. Gutteridge, Dean-street, St. Fin Barrs, Cork, musician and land-surveyor, for certain improvements on the clarionet.—Jan. 19.

ARTICLE XIX.

METEOROLOGICAL TABLE.

1824.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
1st Mon.							
Jan. 1	W	29·49	29·46	48	36	—	
2	N W	30·31	29·49	47	32	—	
3	N W	30·60	30·31	45	28	—	
4	S W	30·62	30·39	40	28	—	
5	S W	30·45	30·35	40	28	—	
6	Var.	30·45	30·35	40	28	—	
7	N W	30·45	30·44	38	30	—	
8	Var.	30·44	30·37	40	32	—	
9	S W	30·37	30·29	45	34	—	
10	S W	30·39	30·21	45	38	—	[illegible]
11	N	30·58	30·39	43	23	—	
12	S W	30·60	30·58	33	19	—	
13	S W	30·60	30·53	31	23	—	
14	N W	30·53	30·48	31	26	—	
15	N W	30·67	30·45	36	26	—	
16	N	30·68	30·66	36	24	—	
17	N W	30·70	30·56	38	24	—	
18	N W	30·56	30·38	40	31	—	
19	W	30·38	30·28	42	36	—	
20	N W	30·28	29·90	42	38	—	·06
21	S W	29·90	29·43	44	38	—	·31
22	S W	29·43	28·98	51	42	—	·16
23	N W	29·85	28·93	48	34	—	
24	N W	30·10	29·85	50	34	—	
25	W	30·24	30·10	54	49	—	
26	W	30·24	29·99	54	41	—	
27	S W	29·99	29·71	54	36	—	·09
28	S W	29·97	29·68	46	35	—	·02
29	N W	30·26	29·97	42	24	—	
30	W	30·26	30·16	38	26	—	
31	S	30·16	30·11	43	24	·87	
		30·70	28·93	54	19	0·87	0·87

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

First Month.—1. Cloudy and fine: boisterous night. 2. Fine: much wind, but calm at sunset: the *Cirrocumulus* has prevailed these two days. 3. Very fine day. 4. Little wind: a dense fog came on suddenly in the forenoon, and the day was misty after. 5—9. Fine days. 10. Fair: some rain in the night. 11. Fine: cloudy. 12, 13, 14. Hoar frost, with foggy nights: a great quantity of rime gradually accumulated on the trees, chiefly on the south side of the branches, presenting a magnificent spectacle. 15. Overcast, p. m. with a little snow: the wind having risen a little, the rime has fallen from the trees unmelted. 17. It is now *winter* under the trees, with a spring-like appearance every where else: the afternoon actually presented the rudiments of a thunder-cloud, succeeded by beautiful *Cirrocumulus* in bars; amidst which the moon rose with the calm lustre of a summer's evening. 19. *Cirrocumulus* above *Cirrostratus* in light beds over the whole sky. 20. *Cumulostratus*: after which *Nimbi* and an overcast sky: wind, with some rain in the night. 21. A hollow wind; with rain, mostly in the night. 22. Overcast: showers: in the night the wind rose, and it blew hard towards morning. 23. Fine morning: *Cirrostratus*, with *Cirrus* aloft: windy. 24—27. Overcast and cloudy. 28. Showery. 29. Fine: *Cumulus*, with *Cirrocumulus*. 30. Hoar frost: very clear at night. 31. Hoar frost: little wind, but with a hollow sound in the trees: very fine day.

RESULTS.

Winds: N, 2; S, 1; SW, 10; W, 5; NW, 11; Var. 2.

Barometer: Mean height

For the month	30·199 inches.
For the lunar period, ending the 22d	30·119
For 14 days, ending the 6th (moon south)	29·874
For 13 days, ending the 19th (moon north)	30·483

Thermometer: Mean height

For the month	36·955°
For the lunar period	37·172
For 30 days, the sun in Capricorn	37·150

Evaporation	0·87 in.
Rain	0·67

Results omitted by an oversight last Month.

Barometer: Mean height

For the lunar period, ending the 24th	29·979 inches.
For 15 days, ending the 10th (moon south)	30·047
For 13 days, ending the 23d (moon north)	29·992

Thermometer:

For 30 days, the sun in Sagittarius	41·156°

Laboratory, Stratford, Second Month, 21, 1824. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

APRIL, 1824.

ARTICLE I.

On Expansions, particularly on those of Glass and Mercury.

By Mr. Crichton.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Glasgow, March* 10, 1824.

A THOROUGH acquaintance with the various laws which regulate the expansive powers of different bodies, is of such acknowledged importance in all experimental researches, that the most eminent scientific men of this and other countries have their names identified with some correction, or new determination, in this branch of knowledge.

So late, however, as 1818, when MM. Dulong and Petit instituted the experiments detailed in their well-known prize memoir, published in the 7th volume of the Annales de Chimie et de Physique, those gentlemen, though in possession of determinations by Roy, Smeaton, Deluc, Lavoisier, Laplace, and many others, nevertheless deemed it expedient to try anew what the expansions were of glass and mercury, as these were to form the basis of all their after investigations.

They begin with finding the absolute dilatation of mercury. Their method of doing this is so ingenious, that very general reliance seems to be placed on the number they assign, $\frac{1}{55{\cdot}5}$ for the dilatation of mercury in thermic unit from the temperature of freezing, to that of boiling water.

MM. Dulong and Petit next proceed to ascertain the apparent dilatation of mercury in glass, which they effect in the usual way, by subjecting a known weight of that fluid, contained in a glass vessel carefully deprived of air and humidity, to the increase of temperature in the same thermic unit; then dividing the weight of the quantity contained in the vessel at freezing, by the weight of the quantity expelled by boiling, they obtain the quotient 64·8, and thence infer, that $\frac{1}{64\cdot 8}$ is the apparent dilatation of mercury in glass.

These very able experimenters justly insist on the great precision of which this method is susceptible. In trials I have made, with vessels holding from 200 to 500 grains, the capillary opening was so small, that a quantity, corresponding to less than $\frac{1}{20}$ of a degree could be detached when expelled; yet minute as this portion is, it was very palpably noted by the balance, which turned with $\frac{1}{1000}$ of a grain.

Every one who knows the difficulty of obtaining tolerably uniform results from pyrometrical measurements, will readily admit the advantages of the method adopted by MM. Dulong and Petit, when with them we recollect, that "dans les mesures directes de dilatation des solides, l'incertitude se trouve triplée en passant de l'expansion linéaire à l'expansion en volume." An error, however, of considerable importance has entered into all computations from the data thus obtained, which unaccountably remains hitherto undetected; for, though that portion of a fluid, detached by increase of temperature, from a glass vessel of known volume, is indeed the fraction *expelled*, yet this fraction (taking the case of MM. Dulong and Petit $\frac{1}{64\cdot 8}$, as "le poids du mercure qui en sortait," does by no means denote the *dilatation* of mercury *in* glass; consequently the dilatation they deduce as that of glass itself must be erroneous.

To illustrate this, let us suppose that a vessel, containing 64·8 parts by weight of a fluid, throws out one of those parts by increase of temperature; it is evident that the dilatation of that one part has not been taken into account, for were it put into another vessel just holding it at 32°, then would another dilatation take place of $\frac{1}{4199\cdot 04}$ of the original volume, and still with this last portion must the operation be repeated, and so on to infinity; the successive expansions resolving themselves into a series, the sum of which is $\frac{1}{63\cdot 8}$, or real dilatation. Or, to take another view of the matter, the vessel having been heated to 212°, there will remain within 63·8 parts; these cooled to 32° leave at top an empty space = 1, which heating to 212° will fill up; this one part, therefore, or $\frac{1}{63\cdot 8}$ of the volume of the mercury, will be alternately occupied by it, or vacant, as the temperature is 32° or 212°; so that weighing in this experiment, serves merely to determine what parts the vessel contains at boiling.

MM. Dulong and Petit having fixed the absolute dilatation of mercury at $\frac{1}{55 \cdot 5}$, and its apparent dilatation in glass at $\frac{1}{64 \cdot 8}$, proceed to determine that of their vessel. But here an error has been committed not less important than the other; for in order to obtain the dilatation in question, they adopt a common but false assumption; that if the absolute dilatation of a fluid, and its apparent dilatation in a vessel be known, the difference between these must represent that of the vessel itself, and of course give $\left(\frac{1}{55 \cdot 5} - \frac{1}{64 \cdot 8}\right) \frac{1}{387}$ as the number; the result here is far from the truth, and would still have been so, though the proper number $\frac{1}{63 \cdot 8}$ had been used instead of $\frac{1}{64 \cdot 8}$; the difference, in neither case, giving the real dilatation which the vessel must have undergone, in the interval of temperature between 32° and 212°.

To learn the true dilatation in this instance, we have only to recollect, that whatever quantity is expelled from a vessel, by a given increase of temperature, something more would be expelled if the vessel itself did not expand; and that this supposed portion must be added to the quantity expelled at the higher temperature (as found by experiment), and deducted from that then remaining in the vessel, that each may represent what it would be, if the vessel were not liable to expansion; the following illustration will furnish a concise general formula, for all similar dilatations of vessels.

To find this correcting quantity, we may take the coefficient of the dilatation of the vessel to express its capacity at any given temperature, as 32°, consequently the same coefficient, plus unity, will express its increased capacity at the higher temperature, 212°.

Now, in the case of MM. Dulong and Petit, if g be that coefficient, g and $g + 1$ will respectively represent the capacities at the given extremes of temperature; and from what is said above, $63 \cdot 8 - \frac{63 \cdot 8}{g+1}$ must be the corrected contents, as $1 + \frac{63 \cdot 8}{g+1}$ is the true expelled quantity at 212°, then, making the former of these divided by the latter = the coefficient of the absolute dilatation of mercury, that is, $\frac{63 \cdot 8\, g}{g + 64 \cdot 8} = 55 \cdot 5$, we obtain $g = 433 \cdot 301$, or $\frac{1}{433 \cdot 301}$ for the absolute dilatation of the vessel used by MM. Dulong and Petit, and not $\frac{1}{387}$ as in the table, Annales, p. 188;

giving, besides, for elongation of a glass rod taken as 1 at freezing, $\frac{1}{1301}$ at boiling water, instead of $\frac{1}{1161}$.

If the above reasoning be conclusive, it necessarily follows, that the corrections for the expansion of glass, as applied to the air and mercurial thermometers, in their after detailed experiments on capacity for caloric, and the relative times of cooling, must, to this extent, have been defective.

The fallacies which MM. Dulong and Petit have inadvertently overlooked, affect not only all calculations where expansion of glass should be attended to, but must have led to false results as to the dilatations of the metals in table 4, p. 141, even though the premises in the preceding page were true, viz. that "le volume sorti représente évidemment la somme des dilatations du mercure et du métal diminuée de la dilatation du verre." Now this formula is manifestly wrong, since the volume driven out does not represent the dilatation of the mercury, and that of the vessel we have shown to be overrated.

By the above corrected method, the absolute dilatation of water, from its state of maximum density at 42·3° to 212°, was found to be $\frac{1}{22{\cdot}76}$. This number is greater than that generally received; but as 39° has sometimes been assumed as the point of greatest density, instead of 42·3°, this circumstance, which is in effect the same as if 45·6° had been adopted, will, to a certain extent, account for the variation.

The same method gave for the dilatation of air by the increase of temperature from 32° to 212°, $\frac{1}{2{\cdot}6546}$, M. Lussac found $\frac{1}{2{\cdot}666}$, though by means which few will think capable of minute precision. The vessel used in my trial was hermetically sealed at the extremes of temperature; this sealing was performed at an opening through a capillary fibre, and to ensure complete dryness, the vessel had been long heated fully to redness just before making the experiment.

In the Memoir, MM. Dulong and Petit particularly mention, that they found all the varieties of glass which they used, to have the same expansive power; this appears surprising, for I can truly affirm, that every specimen of crystal differs more or less from another; trials by mercury give from $\frac{1}{63{\cdot}6}$ to $\frac{1}{65{\cdot}1}$, as the fractions expelled, in the range from freezing, to boiling water, even while the mercury has undergone the same rigorous and repeated boilings, these fractions indicating elongations of glass rods, by that increase of temperature, from $\frac{1}{1261}$ to $\frac{1}{1492}$.

That crystal which is the most colourless is commonly the most ductile and least expansible; but neither from its specific gravity, nor from its tint, as seen through the axis of a tube, can we estimate its expansive power; besides, it is commonly known that tubes of every description, in course of time, become less ductile; hence it is not improbable, that a change takes place in their rates of expansion.

Having been at first induced by the highly sanctioned celebrity of the Memoir to peruse it with a view to establish a proper graduation of the higher part of the thermometric scale, I may, on some future occasion, show, what that graduation ought to be, for the degrees above 212°, and for those below 32°; beyond which two unalterable points, no scale hitherto laid down, gives indications corresponding to those of the degrees within the limits of the primary thermic unit.

Besides, as it must be granted that an error exists in the lower part of the mercurial thermometer, so must it likewise in that filled with spirit of wine, and probably to a greater extent; as perhaps it has never been proved, that the expansive powers of alcohol are, for equal increments of heat, similar to those of mercury, particularly at the low temperatures reported by recent navigators, as having been observed in the polar seas.

Indeed it would be no easy matter, except in climates where very low temperatures prevail, to determine the rates of the spirit thermometer, with reference to the mercurial one; but to attempt doing this by comparing the two instruments as at present constructed, would lead only from one erroneous system of graduation to another.

It will have been perceived that some of the expansions stated above, are greatly less than those given by the best authors. But as the deductions are founded on the supposition that the absolute dilatation of mercury is really $\frac{1}{55 \cdot 5}$; either the legitimacy of these deductions, as now made, or the number itself, may be called in question, if any credit be due to former determinations, by experimenters of very high reputation.

James Crighton.

Article II.

On the Atomic Weight of Boracic and Tartaric Acids. By Thomas Thomson, MD. FRS. Regius Professor of Chemistry in the University of Glasgow.

(To the Editor of the *Annals of Philosophy.*)

Dear Sir,

In the table of the atomic weights of chemical substances which you inserted in the *Annals* for March, 1824, I perceive that you make the atomic weight of boracic acid 2·75, and of tartaric acid 8·375. These numbers being probably derived from my experiments on these acids in the New Series of the *Annals* (vol. ii. p. 131 and 138), it may be proper to state, that by subsequent experiments I have satisfied myself that the true atomic weights of these two acids are as follows:

Boracic acid	3·0
Tartaric acid	8·25

1. By turning to my experiments on boracic acid, you will find that I was not quite satisfied of their accuracy. I was anxious, therefore, to find some method which would be susceptible of greater precision, and found it last summer in fluoboric acid, which is a compound of

Fluoric acid	1·25
Boracic acid	3·0
	4·25

And its atomic weight is 4·25. I had previously determined the atomic weight of fluoric acid to be 1·25, and I knew from my old experiments that the atomic weight of boracic acid was at least as high as 2·75. It is obvious from this that fluoboric acid is a compound of an atom of each of its constituents, and consequently that an atom of boracic acid is 3.

Davy's analysis of the hydrated boracic acid must be nearer the truth than those of Berzelius and my own. I have repeated them again with the same result as before. No doubt some of the boracic acid had made its escape during the application of the heat.

2. My experiments on tartaric acid were made with the crystals of that acid. I began to suspect that these crystals, which are usually large, might contain some water mechanically lodged between their plates. I, therefore, had recourse to tartrate of potash, which contains no water of crystallization, and which may be made anhydrous by exposure to a heat of about 212° for a sufficient time. 14·25 grains of this anhydrous salt were dissolved in water and mixed with a solution of 20·75 grains of nitrate of lead. After the tartrate of lead had precipitated, the supernatant liquid was tested by tartrate of potash, and by nitrate of lead, but was not affected by either. Tartrate of lead is very slightly soluble in water. The consequence of this is, that sulphate of soda when dropped into the supernatant liquid in the above experiment occasions a sensible precipitate.

The crystals of tartaric acid contain 1 atom of water united to 1 atom of acid; hence their true atomic weight is 9·375, and not 9·5 as I stated formerly.

I am, dear Sir, yours truly,

THOMAS THOMSON.

ART. III.—*Corrections in Right Ascension of 37 Stars of the Greenwich Catalogue.* By James South, FRS.

(*Continued from p. 45.*)

Mean AR 1824.	γ Pegasi h. m. s. 0 4 11·17	Polaris h. m. s. 0 58 2·66	α Arietis h. m. s. 1 57 16·42	α Ceti h. m. s. 2 53 5·44	Aldebaran h. m. s. 4 25 50·01	Capella h. m. s. 5 3 42·21	Rigel h. m. s. 5 6 5·11	β Tauri h. m. s. 5 15 10·52	α Orionis h. m. s. 5 45 38·93
April 1	+ 0·52″	−41·51″	+ 0·62″	+ 0·77″	+ 1·23″	+ 1·77″	+ 1·15″	+ 1·64″	+ 1·56″
2	53	41·46	62	77	22	75	13	62	54
3	55	41·41	62	76	21	73	12	61	53
4	56	41·36	63	76	20	71	10	59	51
5	57	41·30	63	76	19	69	09	58	50
6	59	41·25	63	75	18	67	07	56	48
7	60	41·20	63	75	16	66	06	55	46
8	61	41·15	64	74	15	64	04	53	45
9	63	41·10	64	74	14	62	02	51	43
10	64	41·04	64	73	13	60	01	50	42
11	66	40·89	64	73	12	59	00	49	41
12	67	40·75	65	73	11	57	0·99	48	39
13	69	40·60	65	73	10	56	98	47	38
14	70	40·46	66	73	09	54	97	45	37
15	72	40·31	66	73	08	53	96	44	36
16	73	40·10	67	73	07	52	95	43	34
17	75	39·89	67	72	06	50	93	42	33
18	76	39·68	68	72	05	48	92	40	32
19	78	39·47	68	72	04	47	91	39	30
20	80	39·26	69	72	04	46	89	38	29
21	82	38·99	70	72	04	45	88	37	28
22	84	38·72	71	73	03	44	87	36	27
23	86	38·45	72	73	03	43	87	35	27
24	88	38·17	73	74	03	42	86	35	26
25	90	37·90	74	74	03	41	85	34	25
26	92	37·57	75	74	02	40	84	33	24
27	94	37·23	76	75	02	38	83	32	24
28	96	36·90	78	75	02	37	83	31	23
29	98	36·57	79	75	02	36	82	30	22
30	1·00	36·23	80	76	01	35	81	29	21
May 1	02	35·85	81	77	01	35	81	29	20
2	04	35·46	83	78	01	34	80	28	20
3	07	35·08	84	78	01	34	80	28	19
4	09	34·69	85	79	01	33	79	28	19
5	11	34·31	87	80	01	33	79	27	18
6	14	33·86	88	81	01	33	78	27	17
7	16	33·40	89	81	01	32	78	27	17
8	18	32·95	91	82	01	32	78	27	16
9	21	32·50	92	83	01	32	77	27	15
10	23	32·04	94	84	01	31	77	26	14
11	26	31·54	96	85	02	31	77	26	14
12	28	31·04	98	86	02	31	77	26	13
13	31	30·55	1·00	88	02	31	77	26	13
14	34	30·05	02	89	03	31	77	26	13
15	36	29·55	04	90	03	31	77	26	13
16	39	28·99	06	92	04	31	76	27	12
17	42	28·44	08	93	04	32	76	27	12
18	45	27·88	10	94	05	32	76	27	12
19	48	27·33	12	95	05	32	76	27	11
20	50	26·77	14	97	06	32	76	27	11
21	53	26·18	16	99	07	33	76	27	11
22	56	25·59	19	1·00	08	33	77	28	11
23	59	25·01	21	02	09	34	77	28	11
24	62	24·42	23	04	10	35	78	28	12
25	65	23·83	25	05	11	35	78	29	12
26	67	23·18	27	07	12	36	78	29	12
27	70	22·52	30	09	12	36	79	30	12
28	73	21·87	32	10	13	37	79	30	13
29	76	21·21	34	12	14	38	80	31	13
30	79	20·56	37	14	15	39	80	31	13

Mean AR 1824.	Sirius h. m. s. 6 37 25·49	Castor h. m. s. 7 23 21·46	Procyon h. m. s. 7 30 5·32	Pollux h. m. s. 7 34 32·18	α Hydræ h. m. s. 9 18 56·44	Regulus h. m. s. 9 58 59·57	β Leonis h. m. s. 11 40 4·75	β Virginis h. m. s. 11 41 31·85	Spica Virg. h. m. s. 13 15 36·07
April 1	+ 1·47″	+ 2·50″	+ 2·07″	+ 2·48″	+ 2·45″	+ 2·83″	+ 3·06″	+ 3·00″	+ 3·04″
2	45	48	05	46	44	82	06	00	05
3	43	46	04	44	42	81	06	01	06
4	42	45	02	42	41	80	05	01	06
5	40	43	01	41	40	79	05	01	07
6	39	41	1·99	39	38	78	05	02	08
7	38	39	98	37	37	78	05	02	09
8	36	38	96	35	36	76	04	03	10
9	34	36	95	33	34	76	04	03	11
10	31	34	93	31	33	75	04	04	12
11	29	32	91	29	32	74	04	03	13
12	28	31	90	28	30	73	03	02	13
13	26	29	88	26	29	72	03	02	14
14	24	27	87	25	28	71	03	01	14
15	23	25	85	23	27	70	02	00	15
16	21	24	83	22	25	68	02	2·99	15
17	19	22	81	20	24	67	02	99	16
18	18	20	80	19	23	66	01	98	17
19	16	19	79	17	22	65	01	97	17
20	14	17	77	15	20	64	00	96	18
21	13	15	76	13	19	63	2·99	96	18
22	11	14	74	12	17	62	99	95	18
23	10	12	73	10	16	61	98	95	18
24	08	11	72	09	15	59	98	95	19
25	07	09	70	07	14	58	97	94	19
26	06	07	69	06	12	57	97	94	19
27	04	06	67	04	11	56	96	93	19
28	03	04	66	03	10	55	96	93	19
29	01	03	64	01	09	53	95	92	20
30	1·00	01	63	1·99	07	52	94	92	20
May 1	0·99	2·00	62	98	06	51	93	91	20
2	98	1·98	61	97	04	50	92	90	20
3	97	97	60	95	03	49	92	90	20
4	96	96	59	94	02	47	91	89	20
5	95	95	58	93	00	46	90	88	20
6	93	93	57	92	1·99	45	89	87	20
7	92	92	55	91	97	44	88	87	20
8	91	91	54	90	96	43	87	86	20
9	90	90	53	88	94	41	87	85	20
10	89	88	52	87	93	40	86	84	20
11	88	87	51	86	92	39	85	83	20
12	87	86	50	85	91	38	84	82	20
13	86	85	49	84	90	36	83	82	20
14	85	84	48	83	89	35	82	81	20
15	84	83	47	82	88	34	81	80	20
16	84	82	47	81	86	33	81	79	19
17	83	81	46	80	85	32	80	78	19
18	82	79	45	79	84	31	79	78	19
19	81	78	44	78	83	29	78	77	19
20	80	77	43	77	82	28	77	76	19
21	80	76	42	76	81	27	76	75	19
22	79	76	42	75	80	26	75	74	18
23	79	75	41	75	79	25	74	73	18
24	79	75	41	74	78	24	73	72	17
25	78	74	40	73	77	23	72	71	17
26	78	74	40	72	76	21	71	71	16
27	77	73	39	71	75	20	71	70	16
28	77	73	38	71	73	19	70	69	15
29	76	72	38	70	72	18	69	68	14
30	76	71	37	69	70	17	68	67	14
31	76	71	37	69	70	16	67	66	13

Mean AR 1824.	Arcturus	2 α Libræ	α Cor. Bor.	α Serpent.	Antares	α Herculis	α Ophiuchi	α Lyræ	γ Aquilæ
	h. m. s. 14 7 [illegible]	h. m. s. 14 41 9·63	h. m. s. 15 27 14·45	h. m. s. 15 35 36·47	h. m. s. 16 18 17·91	h. m. s. 17 6 37·72	h. m. s. 17 26 46·24	h. m. s. 18 30 [illegible]	h. m. s. 19 37 [illegible]
April 1	+ 2·91″	+ 2·03″	+ 2·60″	+ 2·66″	+ 2·96″	+ 2·17″	+ 2·09″	+ 1·30″	+ 1·41″
2	93	05	62	68	3·00	20	12	33	44
3	94	06	64	70	03	22	14	37	47
4	95	08	66	72	07	25	17	40	50
5	96	10	68	74	11	27	20	43	53
6	97	11	70	76	15	30	23	47	55
7	99	13	73	79	19	32	25	50	58
8	3·00	15	75	81	22	35	28	53	61
9	02	16	77	83	26	38	30	57	64
10	03	18	79	85	30	40	33	60	67
11	04	19	81	87	33	42	35	63	70
12	05	21	82	88	35	45	38	66	73
13	06	22	84	90	38	47	40	69	75
14	07	23	86	91	40	50	43	72	78
15	08	25	87	93	43	52	45	75	81
16	08	26	89	95	45	55	48	78	84
17	09	27	90	96	48	57	50	81	87
18	10	29	92	98	51	60	53	85	89
19	11	30	94	3·00	54	62	55	88	92
20	12	32	96	02	56	65	58	92	95
21	13	33	97	04	58	67	60	95	98
22	13	34	99	05	60	69	62	98	2·01
23	14	36	3·00	07	63	72	64	2·01	04
24	14	37	02	08	65	74	66	04	07
25	15	38	03	10	67	76	69	07	10
26	16	39	04	11	69	78	71	10	13
27	16	40	06	13	71	81	73	13	16
28	17	42	07	14	74	83	75	16	19
29	17	43	09	16	76	85	77	19	22
30	18	44	10	18	78	87	79	22	25
May 1	18	45	11	19	80	89	81	25	28
2	19	46	12	20	82	91	84	28	31
3	19	46	13	22	84	93	86	31	34
4	19	47	14	23	86	95	88	34	37
5	20	48	15	24	88	97	91	37	40
6	20	49	16	25	90	99	93	40	42
7	20	49	18	26	92	3·01	96	43	45
8	20	50	19	27	94	03	98	46	48
9	21	51	20	29	96	05	3·01	49	51
10	21	52	21	30	98	07	03	52	54
11	21	53	22	31	4·00	09	05	54	57
12	21	53	23	32	01	10	07	57	59
13	21	54	23	33	03	12	09	59	62
14	21	55	24	34	04	14	11	62	65
15	21	55	25	35	06	15	13	64	68
16	22	56	26	36	08	17	14	67	70
17	22	57	27	37	09	19	16	69	73
18	22	57	27	38	11	21	18	72	76
19	22	58	28	39	12	22	20	74	79
20	22	59	29	40	14	24	22	77	81
21	22	59	29	41	15	25	24	79	84
22	22	59	30	41	17	27	25	81	86
23	21	59	30	42	18	28	27	84	89
24	21	60	30	43	19	30	29	86	91
25	21	60	31	43	20	31	31	88	94
26	19	60	31	44	22	33	32	90	96
27	19	60	32	44	23	34	34	92	99
28	19	61	32	45	24	36	36	94	3·02
29	19	61	32	46	26	37	38	97	04
30	19	61	33	47	27	39	39	99	07
31	19	61	33	47	28	40	40	3·01	09

Mean AR 1824.	α Aquilæ h. m. s. 19 42 11·68	β Aquilæ h. m. s. 19 46 40·28	2 α Capric. h. m. s. 20 8 17·02	α Cygni h. m. s. 20 35 26·21	α Aquarii h. m. s. 21 56 44·67	Fomalhaut h. m. s. 22 47 54·84	α Pegasi h. m. s. 22 56 0·17	α Androm. h. m. s. 23 59 18·67
April 1	+ 1·42″	+ 1·42″	+ 1·56″	+ 0·32″	+ 0·92″	+ ·81″	+ 0·59″	+ 0·36″
2	45	45	59	35	95	83	62	37
3	48	48	62	39	97	86	65	39
4	51	51	65	42	99	88	68	40
5	53	53	68	46	1·01	91	71	41
6	56	56	70	49	04	93	74	43
7	59	59	73	52	06	95	77	44
8	62	62	76	56	08	97	79	45
9	64	65	79	60	11	1·00	82	47
10	67	68	82	63	13	02	75	48
11	70	71	85	67	15	04	77	50
12	73	74	88	70	18	07	79	52
13	76	76	91	74	20	09	82	53
14	79	79	94	77	23	12	84	55
15	82	82	97	81	25	14	86	56
16	84	85	2·01	84	28	16	88	58
17	87	88	04	88	30	19	90	60
18	90	91	07	92	33	21	92	62
19	93	93	10	95	35	24	95	64
20	96	96	13	99	38	26	97	66
21	99	99	16	1·03	41	29	99	68
22	2·02	2·02	19	06	43	32	1·02	70
23	05	05	22	10	46	35	04	72
24	08	08	25	14	49	38	07	74
25	11	11	28	17	51	41	09	77
26	14	14	31	21	54	43	12	79
27	17	17	34	25	57	46	14	81
28	20	20	37	28	60	49	17	83
29	23	23	40	32	62	52	19	85
30	26	26	43	36	65	55	22	87
May 1	29	29	46	40	68	58	25	90
2	32	32	50	43	71	61	28	92
3	35	35	53	47	74	64	30	95
4	38	38	56	51	77	67	33	97
5	41	41	59	55	80	70	36	1·00
6	43	43	63	58	83	73	39	02
7	46	46	66	62	86	76	42	05
8	49	49	69	66	89	79	44	07
9	52	52	73	69	92	82	47	10
10	55	55	76	73	95	85	50	13
11	58	58	79	77	98	88	53	16
12	61	61	82	80	2·01	92	56	19
13	63	63	85	84	04	95	59	22
14	66	66	88	87	07	98	62	25
15	69	69	91	91	10	2·01	65	27
16	72	72	93	94	13	04	68	30
17	75	75	96	98	16	08	71	33
18	77	77	99	2·01	19	11	74	36
19	80	80	3·02	05	22	15	77	39
20	83	83	05	09	25	18	80	42
21	86	86	08	12	28	21	83	45
22	88	88	11	16	31	25	86	48
23	91	91	14	19	34	28	89	51
24	93	94	17	23	37	32	92	54
25	96	97	20	26	40	35	95	58
26	98	99	22	30	43	38	98	61
27	3·01	3·02	25	33	46	42	2·01	64
28	04	03	28	37	49	45	04	67
29	06	08	31	40	52	49	07	70
30	09	10	34	44	55	52	10	73
31	11	12	37	47	58	55	13	76

	γ Pegasi	Polaris	α Arietis	α Ceti	Aldebaran	Capella	Rigel	β Tauri	α Orionis
Mean AR 1824.	h. m. s. 0 4 41·17	h. m. s. 0 58 2·66	h. m. s. 1 57 16·42	h. m. s. 2 53 5·44	h. m. s. 4 25 50·01	h. m. s. 5 3 42·21	h. m. s. 5 6 5·11	h. m. s. 5 15 10·52	h. m. s. 5 45 38·9
June 1	+ 1·85″	−19·23″	+ 1·42″	+ 1·18″	+ 1·18″	+ 1·41″	+ 0·82″	+ 1·33″	+ 1·14″
2	88	18·57	45	20	19	43	83	34	14
3	91	17·90	48	22	21	44	84	35	15
4	94	17·24	50	24	22	45	85	36	15
5	97	16·52	53	26	24	47	86	37	16
6	2·00	15·80	55	28	25	48	87	39	16
7	03	15·09	58	30	27	49	88	40	17
8	06	14·37	61	33	28	51	89	41	17
9	09	13·65	64	35	30	52	90	42	18
10	12	12·92	67	37	32	54	91	43	19
11	16	12·19	70	40	34	56	93	45	20
12	19	11·46	73	42	35	58	94	46	21
13	22	10·73	76	45	37	60	95	48	22
14	25	10·00	79	48	39	62	96	49	23
15	29	9·24	82	50	40	63	98	51	24
16	32	8·48	85	52	42	65	99	52	26
17	35	7·72	88	55	44	67	1·00	54	27
18	39	6·96	91	57	46	69	01	55	28
19	42	6·20	94	60	48	71	03	57	29
20	45	5·41	97	63	50	73	05	59	30
21	48	4·62	2·00	65	53	76	06	61	32
22	51	3·83	03	68	55	78	08	63	33
23	54	3·04	07	70	57	81	10	65	35
24	57	2·25	10	73	59	83	11	67	36
25	60	1·47	13	75	62	86	13	69	38
26	63	0·68	16	78	64	88	15	71	39
27	67	+ 0·11	19	81	66	90	16	73	41
28	70	0·89	23	84	69	92	18	75	42
29	73	1·68	26	86	71	95	20	77	44
30	76	2·48	29	89	73	98	22	79	46

	Sirius	Castor	Procyon	Pollux	α Hydræ	Regulus	β Leonis	β Virginis	Spica Virg.
Mean AR 1824.	h. m. s. 6 37 28·49	h. m. s. 7 23 21·46	h. m. s. 7 30 5·82	h. m. s. 7 34 32·18	h. m. s. 9 18 56·44	h. m. s. 9 58 59·57	h. m. s. 11 40 4·73	h. m. s. 11 41 31·86	h. m. s. 13 15 56·07
June 1	+ 0·76″	+ 1·71″	+ 1·36″	+ 1·68″	+ 1·69″	+ 2·15″	+ 2·66″	+ 2·65″	+ 3·13″
2	76	71	36	68	68	14	65	64	12
3	76	71	36	68	67	13	64	63	12
4	76	70	35	67	66	12	63	62	11
5	76	70	35	67	65	11	62	62	11
6	76	70	35	67	64	10	61	61	10
7	76	70	35	67	63	09	60	60	09
8	76	69	34	66	63	08	59	59	09
9	76	69	34	66	62	07	58	58	08
10	76	69	34	66	61	06	57	57	07
11	77	69	34	66	61	06	56	56	07
12	77	69	34	66	60	05	55	55	06
13	78	70	34	66	60	04	54	54	05
14	78	70	34	66	59	03	53	53	04
15	78	70	35	66	58	03	52	52	04
16	79	70	35	67	57	02	50	51	03
17	79	71	35	67	57	01	49	50	02
18	80	71	35	67	56	00	48	49	02
19	80	71	35	67	56	00	47	48	01
20	81	71	35	67	56	1·99	46	47	00
21	81	72	36	68	55	99	45	46	00
22	82	72	36	68	55	98	44	45	2·99
23	83	73	37	69	54	98	43	44	98
24	84	73	37	69	54	97	42	43	97
25	84	74	38	70	53	97	41	43	96
26	85	74	38	70	53	96	40	42	95
27	86	75	39	71	52	95	39	41	95
28	86	76	39	72	52	94	38	40	94
29	87	76	40	72	51	94	37	39	93

	Arcturus	2 α Libræ	α Cor. Bor.	α Serpent.	Antares	α Herculis	α Ophiuchi	α Lyræ	γ Aquilæ
Mean AR 1824.	h. m. s. 14 7 26·83	h. m. s. 14 41 9·63	h. m. s. 15 27 14·45	h. m. s. 15 35 36·47	h. m. s. 16 18 37·91	h. m. s. 17 6 37·72	h. m. s. 17 26 46·24	h. m. s. 18 30 58·99	h. m. s. 19 37 56·68
June 1	+ 3·18″	+ 3·61″	+ 3·33″	+ 3·48″	+ 4·29″	+ 3·41″	+ 3·42″	+ 3·03″	+ 3·12″
2	18	61	33	48	30	42	43	05	14
3	17	61	33	49	31	43	44	07	17
4	17	61	33	49	32	44	46	09	19
5	17	61	33	49	33	45	47	10	22
6	16	62	33	49	34	47	48	12	24
7	16	62	34	50	35	48	49	14	27
8	15	62	34	50	36	49	51	16	29
9	15	62	34	51	37	50	52	18	32
10	14	62	34	51	37	51	53	19	34
11	14	61	33	51	38	52	54	21	36
12	13	61	33	51	38	52	55	22	38
13	12	61	33	51	39	53	56	24	40
14	11	61	33	51	39	54	57	25	43
15	11	60	32	51	40	55	58	26	45
16	10	60	32	52	40	56	59	28	47
17	09	60	32	52	41	57	60	29	49
18	09	59	31	52	41	57	61	31	51
19	08	59	31	52	42	58	62	32	53
20	07	59	30	52	42	58	63	33	55
21	06	58	30	51	43	59	63	34	57
22	06	58	29	51	43	59	64	35	58
23	05	57	29	51	43	59	64	36	60
24	04	57	28	51	43	60	65	37	62
25	03	56	28	50	43	60	65	37	64
26	02	56	27	50	44	61	66	38	66
27	01	55	27	49	44	61	66	39	68
28	00	55	26	49	44	62	67	40	69
29	2·99	54	25	49	45	62	68	41	71
30	98	53	24	49	45	62	68	42	72

	α Aquilæ	β Aquilæ	2 α Capricor.	α Cygni	α Aquarii	Fomalhaut	α Pegasi	α Androm.
Mean AR 1824.	h. m. s. 19 42 11·88	h. m. s. 19 46 40·23	h. m. s. 20 8 17·02	h. m. s. 20 35 26·21	h. m. s. 21 56 44·67	h. m. s. 22 47 54·54	h. m. s. 22 55 0·37	h. m. s. 23 59 12·67
June 1	+ 3·14″	+ 3·15″	+ 3·39″	+ 2·50″	+ 2·61″	+ 2·59″	+ 2·16″	+ 1·80″
2	16	17	42	53	64	62	19	83
3	19	20	45	56	67	66	22	86
4	21	22	48	59	71	69	25	89
5	24	25	51	62	74	73	28	93
6	26	27	53	66	77	76	32	96
7	29	30	56	69	80	80	35	99
8	31	33	59	72	84	83	38	2·02
9	34	35	62	75	87	87	41	06
10	36	37	64	78	90	90	44	09
11	38	39	67	81	93	94	48	13
12	40	41	69	84	96	97	51	16
13	43	44	72	87	99	3·01	54	20
14	45	46	74	90	3·02	04	57	23
15	47	48	77	93	04	07	61	27
16	49	50	79	95	07	11	64	30
17	52	53	82	98	10	14	67	34
18	54	55	84	3·01	13	18	71	37
19	56	57	87	04	16	21	74	41
20	58	59	89	06	19	24	77	44
21	60	61	91	09	22	28	80	48
22	61	62	93	11	24	31	83	51
23	63	64	96	14	27	35	86	54
24	65	66	98	16	30	38	89	58
25	67	68	4·00	18	33	41	92	61
26	68	70	02	21	36	45	95	64
27	70	71	04	23	38	48	98	68
28	72	73	07	26	41	52	3·01	71
29	74	75	09	28	44	55	04	74
30	75	77	11	30	47	58	07	77

ARTICLE IV.

*A Contribution to a more accurate Knowledge of Uranium.**

By J. A. Arfwedson.

URANIUM in the state of an oxide is occasionally found native pretty pure, as, for example, in uran ochre and uran mica; but the scarcity of these minerals has prevented them from being employed for preparing oxide of uranium in any considerable quantity. Chemists have, therefore, in their researches on this metal, been obliged to employ the more abundant mineral called *pechblende*, in which oxide of uranium is likewise found, mixed with several other bodies from which it is with difficulty separated.

Klaproth found pechblende, from Joachimsthal, in Bohemia, to contain, together with protoxide of uranium, silica, oxide of iron, and sulphuret of lead. He extracted the protoxide of uranium in the following way: The pulverised mineral was dissolved in nitric acid, the silica and sulphur remaining undissolved. From the filtered solution was separated by crystallization in the first place, the lead, under the form of nitrate of lead. The liquid being further evaporated, the nitrate of uranium crystallized, which was finally decomposed by caustic potash. The oxide of iron remained in the mother ley.

Bucholz prepared his oxide of uranium in this manner.—The pulverised pechblende was boiled with nitric acid, as long as any thing was dissolved. The solution was evaporated at a high temperature till fumes of nitrous acid were extricated, and this was continued for a considerable time, taking care to stir the matter continually. The salt of uranium was then taken up by water, while the oxide of iron remained behind undissolved. Bucholz found, however, that the solution contained likewise copper and lime; which were separated in this manner. The liquid was decomposed by caustic ammonia added in excess, and digested for some time along with the precipitate. By this process the precipitate was freed from copper. It was washed and heated to redness to drive off the whole of the ammonia. It was again dissolved in nitric acid, and precipitated by caustic potash, added as little as possible in excess. The oxide of uranium thus obtained retained its yellow colour after being heated to redness, and was considered as free both from lime and potash.

From the knowledge of the subject which we at present possess, it is easy to see that neither Klaproth nor Bucholz could have obtained a perfectly pure oxide of uranium, on account of the great variety of substances accidentally present in pechblende, the names of which I shall state below.

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* Translated from the Kongl. Vetenskaps Academiens Handlingar, for 1822, p. 404.

Method of preparing pure Oxide of Uranium.

I expected at first to have been able to obtain pure oxide of uranium without any tedious processes, by means of one of its properties which has not been known for any considerable time; namely, that it dissolves with facility in carbonate of ammonia, and is again precipitated from the solution by boiling; for should some copper accompany it in the solution, the oxide of uranium which first is precipitated ought to be quite free from all admixture of that metal.

A portion of pechblende from Johann Georgenstadt in Saxony, which was apparently very pure, was reduced to a fine powder, and digested with nitric acid till it was completely decomposed. After this a little muriatic acid was added, which dissolved a considerable portion of the straw-yellow matter, insoluble in nitric acid. The filtered solution was supersaturated with carbonate of ammonia in great excess. A portion of the precipitate was again dissolved by the carbonate, but the greatest part remained undissolved; and this proportion continued unaltered though the whole was heated. The ammoniacal solution was separated from the precipitate, and was examined by means of processes which I consider it unnecessary to particularize in this place, and to my astonishment I found it to contain the following different substances; namely, oxide of uranium, oxide of copper, a considerable proportion of oxide of cobalt, a little oxide of zinc; and the precipitate, besides all these bodies, contained much arsenic mixed with oxides of iron and lead. If to these we add the silica and sulphur not dissolved by the acids, we shall find that the pechblende contains no fewer than nine different substances. To free oxide of uranium from so many other bodies occasioned a great number of fruitless trials. But I at last succeeded in discovering the following method, by means of which, so far as I can see, the protoxide of uranium may be obtained in a state of complete purity.

Finely pulverized pechblende is dissolved by means of a gentle heat in a mixture of nitric and muriatic acids. When the decomposition of the mineral is completed, and most of the acid expelled, a little muriatic acid is to be added, after which the liquid is to be diluted with a good deal of water. The sulphur, silica, and a portion of the gangue, remain finally undissolved. A current of sulphuretted hydrogen gas must now be passed through the liquid as long as any precipitate continues to fall. The precipitate is at first dark-brown, consisting of sulphurets of copper, arsenic, and lead; but at last it becomes yellow, and consists of sulphuret of arsenic. The liquid is now free from copper, lead, and arsenic, but it contains iron, cobalt, and a little zinc.

Let it be filtered and digested with a little additional nitric acid to peroxidize the iron. By this process the light green colour of the liquid changes to a yellow. It must now be decomposed by means of carbonate of ammonia added in excess, which will take up the oxide of uranium mixed with oxides of cobalt and zinc, but leaves a great quantity of oxide of iron undissolved. Should the solution even contain a portion of earth, which did not happen in my experiments, almost the whole of it would be separated mixed with the oxide of iron. The filtered solution is afterwards made to boil, and the boiling is continued as long as the carbonate of ammonia is disengaged. A portion of the oxide of cobalt remains in the solution, which acquires a faint reddish colour; but another portion of it is precipitated along with the oxide of uranium; which contains likewise the zinc. The precipitate is collected on the filter, washed, and dried. It is then to be heated to redness; by which it loses its yellow colour, and becomes dark-green. In this state it must be macerated for some time in dilute muriatic acid, which dissolves the oxides of cobalt and zinc, together with a small portion of peroxide of uranium. This portion was probably united as an acid with the two bases. Pure protoxide of uranium remains undissolved. If the muriatic solution be precipitated with caustic ammonia in excess, we obtain oxide of uranium combined with oxides of cobalt and zinc. From 38¼ parts of pechblende treated in this way, I obtained about 25 parts of protoxide of uranium. This amounts nearly to 65 per cent. which is 15 parts less than the quantity stated by Klaproth.

Metallic Uranium and Protoxide of Uranium.

The experiments hitherto made to obtain uranium in the metallic form have been all conducted in charcoal crucibles with or without additions. It is consequently probable, even if the oxide of uranium operated upon had been quite pure, that the metal obtained contained charcoal, or some other substance derived from the fluxes employed in the reduction. In which case the properties of the product might differ materially from those of the pure metal. Fortunately chemists have found out a method of avoiding these inconveniences in the reduction of metals; for it is now known that several metallic oxides may be deprived of their oxygen by means of hydrogen gas. I determined, therefore, to try whether oxide of uranium could not be brought to the metallic state by this method. If I succeeded I obtained naturally the proportion of oxygen in the oxide determined with the requisite exactness.

The apparatus employed in this process was a piece of a common barometer tube, blown into a small globe about the middle part. This tube was in the first place weighed, and then a portion of finely pulverized protoxide of uranium which had been heated to redness was introduced into the globular part of the tube. Before determining the weight of this powder, the glass was heated by a spirit lamp, in order to drive off any moisture that might have been adhering to the powder, and this moisture was sucked out of the tube by the mouth. The tube was then placed in continuity with an apparatus from which hydrogen gas was extricated from a mixture of zinc and dilute sulphuric acid. This gas was made to pass in the first place through a tube filled with fused muriate of lime in order to dry it. It then entered into the tube containing the protoxide of uranium; and as soon as it had expelled the atmospheric air, heat was applied to the protoxide by means of an Argand's spirit lamp. The reduction took place immediately, and with such violence that the matter became red-hot. Water was generated, and at the end of the process, which only lasted a few minutes, the green protoxide was changed into a powder of a liver-brown colour. 1·187 parts of protoxide of uranium had lost by this process 0·042 part, which amounts to 3·53 per cent. In another experiment 1·468 lost 0·052, which amounts to 3·54 per cent. The experiment was repeated once more in a porcelain tube which was heated to whiteness; but the product was the same brown powder.

This substance remains unaltered at the ordinary temperature of the atmosphere; but when heated to the commencement of redness, it takes fire, swells, and is converted into green oxide. It is insoluble in sulphuric and muriatic acids, whether concentrated or diluted; but it dissolves with facility in nitric acid with the evolution of nitrous gas, and the solution has a lemon-yellow colour. It is exceedingly probable that the protoxide of uranium is by this treatment reduced to the metallic state; but it is certainly possible that I only reduced it to a lower state of oxidizement.

Meanwhile I undertook some experiments in order to determine the composition of the yellow oxide of uranium, by means of which I expected to be able to throw some light on the question, whether the substance obtained in the preceding experiments should be considered as a metal or not. If I could prepare a neutral salt with peroxide of uranium and sulphuric or muriatic acid, I should have an easy way of determining the quantity of oxygen in the oxide by the analysis of the salt; but neither of these salts could be obtained in the state of crystals; for on evaporating the solutions, I obtained at last a thick syrup, which, when further evaporated, became greenish-yellow from the formation of protoxide of uranium. On the other hand, when I added to the permuriate of uranium a portion of muriate of potash, a triple salt separated on evaporating the liquid in small lemon-yellow crystals. Since hydrogen gas reduces the protoxide of uranium with such facility, I thought it likely that this triple salt might also be decomposed by means of it, and that its analysis could be best performed in the way that Berzelius proceeded with the analysis of potash-muriate of platinum;

for potash-muriate of uranium may be freed from water without the least difficulty, as the salt can be exposed to a moderate red heat without undergoing any decomposition.

The experiment was conducted in the first place in an apparatus similar to that above described. As soon as the hydrogen gas began to pass, and the salt became hot, it fused and swelled up, muriatic acid gas was given out, and the mass became dark-coloured and opaque. Though the process was continued for nearly two hours, and the heat from the Argand's spirit lamp was raised to the highest degree of intensity, vapours of muriatic acid still continued to pass; a proof that the salt was not completely decomposed. The whole being allowed to cool, the matter was extracted by water, which dissolved muriate of potash, together with a good portion of uranium salt, of a light-green colour. The insoluble residue was a black crystallized powder having the metallic lustre, which was washed, and dried upon blotting paper.

Supposing that the heat in this experiment might have been too weak, and that the salt, if exposed to a higher temperature, might have been more completely decomposed, it was again repeated with this difference, that the salt was put into an apparatus, which could be more strongly heated, and which was introduced half way into a small furnace heated with charcoal. The heat applied was so strong that the glass was almost melted; yet the salt was not completely decomposed; for after washing the altered salt with water, there remained the same crystallized matter as in the preceding experiment; but of a still more decided metallic appearance; for the salt had been employed in greater quantity, and on that account the crystals were larger and more distinct. The form of these crystals as seen under the microscope was an octahedron very nearly regular, whose faces had a very strong metallic lustre. Some of them were slightly transparent at the edges, and of a reddish-brown colour; and this colour remained even after the crystals were reduced to powder. These crystals were not altered by exposure to the atmosphere; but when heated, they were reduced to powder, increased in bulk, and were changed into green protoxide of uranium, which, when treated with acids, exhibited the very same characters as the products of the reduced protoxide.

It is scarcely possible to think, that in this experiment an oxidized body could have been obtained; especially if the double salt employed be viewed according to the new theory of the nature of muriatic acid, according to which that acid contains no oxygen whatever. All the circumstances taken together lead to the conclusion, that the crystallized body obtained was metallic uranium. 0·636 part of it were heated in a platinum vessel, and converted into green oxide. The increase of weight was 0·0235, or 100 parts of the metal had combined with 3·695 parts of oxygen. For the sake of security the oxide was dissolved in nitric acid, the solution evaporated to dryness, and exposed to a red heat; but by this process no alteration whatever of the weight was produced.

The experiment was repeated with 1·006 gramme of metal, which increased in weight 0·0375 gramme, corresponding to 3·73 oxygen to the 100 of metal.

These two experiments agreeing very well with each other show plainly that the brown substance obtained when protoxide of uranium was reduced by means of hydrogen gas, must likewise be in the metallic state. A hundred parts of the protoxide lost 3·53 or 3·54 of their weight, leaving a remainder amounting to 96·47 or 96·46. But 96·46 : 3·54 :: 100 : 3·67, which loss quite corresponds with the augmentation of weight of the metal when heated to redness. It was formerly remarked, that the metal when obtained by reducing the protoxide by means of hydrogen gas has a liver-brown colour; while on the other hand the powder of the crystallized product is reddish-brown; but this may be owing to no other cause than a difference in the fineness of the two powders.

If the result of the reduction of the protoxide be compared with that which is obtained by the combustion of the metal, 100 parts of protoxide of uranium are composed at a medium of

Uranium	96·443
Oxygen	3·557
	100·000

and 100 parts of uranium combine with 3·688 parts of oxygen.

The protoxide of uranium obtained from percarbonate is a powder of a dirty-green colour. If the uranium salt be a second time thrown down by caustic ammonia, and the precipitate be heated to redness, the protoxide is obtained in the form of a black metallic mass, the particles of which cohere together. This mass is exceedingly hard, and is not without difficulty reduced to powder. The powder has the usual green colour of protoxide of uranium. Protoxide of uranium after having been heated to redness, dissolves very sparingly in dilute muriatic or sulphuric acid. The concentrated acids dissolve it better, and when it is boiled in concentrated sulphuric acid, it dissolves completely, a light-green coloured saline mass is obtained, which dissolves in water with a deep bottle-green colour. If such a solution be precipitated by caustic ammonia, the protoxide is separated in the state of a hydrate, in brown flocks inclining to purple. If these flocks be washed and dried at the temperature of 212°, and then heated in a glass tube, they give out a considerable portion of water, and become green. In general a portion of the hydrate is likewise converted into peroxide, and it becomes yellow before the end of the drying; and if it was precipitated by ammonia in great excess, or if it was washed with hot water, in either case we may generally expect to find the whole after being dried in the state of peroxide containing ammonia. Carbonate of ammonia throws down a light-green precipitate of protocarbonate of uranium, which is again dissolved if the precipitant be added in excess. If the precipitate be heated in ammonia, we obtain protoxide of uranium free from carbonic acid. The hydrated protoxide dissolves very easily in acids; and the precipitate by means of caustic ammonia is easily dissolved again if a little excess of acid be added to the liquid; but if the precipitated hydrate be digested for an hour in water, the chemically combined water is disengaged, the matter concretes into a heavy powder of small bulk, and is afterwards acted upon with great difficulty by acids.

Yellow Oxide of Uranium.

Peroxide of uranium, as is well known, has the property of acting sometimes the part of an acid, and at others that of an alkali; and it has such a tendency to enter into combination with other oxidized bodies, that I doubt the possibility of obtaining it in an insulated state. For example, if we precipitate a solution of this oxide in muriatic or nitric acid by means of caustic ammonia, the precipitate is a chemical combination of peroxide of uranium with water and ammonia, which last body cannot be removed by washing the powder. The very same result is obtained, if we precipitate the peroxide by means of caustic potash. The hydrated uraniate of ammonia may be heated without undergoing decomposition to 212°, or a little higher. When raised to a still higher temperature, it gives out water, azotic gas, and ammonia, and protoxide of uranium remains behind. If we attempt on the other hand to heat pernitrate of uranium in order to disengage the acid, the decomposition of the salt does not cease till the whole mass is converted into protoxide; and this result takes place in what way soever we regulate the temperature.

Considering the small quantity of oxygen in the protoxide of uranium, it was of the utmost importance in determining the quantity of oxygen in the peroxide to employ a method which was not liable to any uncertainty. It occurred to me that I should obtain such a method if I combined peroxide of uranium with a basis, the proportion of whose oxygen was accurately known; and then, by means of hydrogen gas, deprived both substances of their oxygen. Knowing the proportions of peroxide of uranium and basis, and the proportion of oxygen on the basis, we should have the oxygen in the peroxide. To enable me to employ such a method, some preliminary experiments were undertaken, by which I found that when to a solution of peroxide of uranium in muriatic acid any earthy or metallic muriate is added, and then caustic ammonia is added to the mixture, in all such cases the peroxide of uranium is precipitated in combination with the earth or metallic oxide in the form of a uraniate, even when the base consists of a substance, which, when in an insulated state, is not precipitated by ammonia; for example, when it is lime or barytes; and in this way a whole series of uraniates may be obtained, which, however, do not resemble other salts in their composition, as will be seen more particularly hereafter. When peroxide of uranium is united to a basis capable of withstanding the fire, it can resist a high temperature without losing any of its oxygen. When, on the contrary, I examined a uraniate having an easily reducible basis, the composition of which was known before, I in that case first reduced the salt by means of hydrogen gas, which gave me the quantity of oxygen contained in both oxides; then determining the quantity of oxygen in the basis I had that in the peroxide of uranium. I employed for this purpose uraniate of lead as most suitable to this kind of investigation.

Analysis of Uraniate of Lead.

The salt was prepared in this manner. Solutions of pernitrate of uranium and nitrate of lead were mixed together, and precipitated by caustic ammonia. The precipitate thus obtained was washed, and exposed to a red heat. It probably contained an excess of oxide of lead, in the form of subnitrate, as the nitrate of lead had been added in considerable excess; but this was of no consequence. The precipitated compound after being heated to redness and pulverized, had a cinnamon-brown colour, and it gave a full lemon-yellow solution in muriatic acid; showing that the peroxide of uranium had lost none of its oxygen.

1·969 gramme of uraniate of lead was reduced by means of hydrogen gas in the same way as the analysis of the protoxide of uranium was performed. As soon as it began to be red hot, it gave out much water, and when this ceased the process was stopped. The product consisted of a dark-brown powder, which weighed 0·127 less than the uraniate of lead. But this difference in weight could not be determined with accuracy, because the apparatus while weighing was constantly increasing in weight. The reduced mass became at the same time hot, and when thrown upon paper, it took fire, and became quite ignited, leaving uraniate of lead as a residue.

This singular phenomenon, owing probably to the rapidity with which the alloy of uranium and lead absorbed oxygen, was so much the more unlooked for, as these metals, when in a separate state, do not undergo any change in the common temperature of the atmosphere. There might, in this case, have been produced an electro-chemical process between them, which occasioned their combustion. Meanwhile no accurate conclusion could be drawn respecting the oxygen which the two metals contained. The experiment, therefore, was repeated, with this alteration, that the water was collected in a receiver, filled with fused muriate of lime, previously exactly balanced to determine its weight.

From 2·3 grammes of uraniate of lead, previously heated to redness, were obtained in this way 0·164 gr. of water, equivalent to 0·1459 oxygen.*

0·628 gramme of uraniate of lead, prepared at the same time, were dissolved in nitric acid. The solution was mixed with sulphuric acid in sufficient quantity to saturate the oxide of lead, and then evaporated almost to dryness. I found it necessary to dissolve the uraniate of lead in the first place in nitric acid; for if it be decomposed directly by sulphuric acid, it is not possible to obtain the sulphate of lead white, and quite free from oxide of uranium. The mass was finally digested in alcohol, which dissolved the sulphate of uranium, and left the sulphate of lead. This last salt was collected on a filter, and washed with alcohol. After being heated to redness, it weighed 0·485 gr. which corresponds with 0·357 protoxide of lead. The remainder of the 0·628 amounting to 0·271 was of course peroxide of uranium. Thus it appears that 2·3 uraniate of lead consist of 1·307 protoxide of lead, and 0·993 of peroxide of uranium. The oxygen in the former of these constituents is 0·0937; but the oxides of lead and uranium had together lost 0·1459 of oxygen; and consequently 0·0522 of oxygen must have belonged to the peroxide of uranium. It follows ultimately that 100 parts of peroxide of uranium contain 5·252 of oxygen.

The experiment was repeated with a uraniate of lead of another preparation; because all the first stock was exhausted.

1·26 gr. reduced by means of hydrogen gas, gave 0·0786 water, corresponding with 0·0698 oxygen; 1·258 gr. of the salt was decomposed by sulphuric acid, and was found to be a compound of 0·173 protoxide of lead, and 1·085 peroxide of uranium. 1·26 gr. of the uraniate of course consist of 0·1733 protoxide of lead and 1·0867 peroxide of uranium; which together contain 0·0698 oxygen; but 0·1733 protoxide of lead contain 0·0124 oxygen. Thus it appears that the oxygen in 1·0867 peroxide of uranium is 0·0574; and 100 parts of peroxide of uranium contain 5·282 oxygen. The preceding experiment gave 5·252; the mean of both is 5·267. From this it follows, that 100 parts of uranium, in order to become peroxide, must combine with 5·560 oxygen.

But that the whole might not depend upon a single set of experiments, I determined likewise to analyze

Uraniate of Barytes.

It was prepared in this manner. A mixture of the solutions of permuriate of uranium and muriate of barytes, both previously boiled, was precipitated by caustic ammonia. The precipitate was collected on a filter, and hastily washed with fresh boiled water, to prevent any mixture of carbonate of barytes. The uraniate of barytes, after being dried and heated to redness, had a fine yellow colour, which became lemon-yellow when the salt was reduced to powder.

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* The oxygen in the water was reckoned 88·94 per cent. according to the experiments of Berzelius and Dulong.

1·343 gramme of uraniate of barytes previously heated to redness, being dissolved in nitric acid, and decomposed by means of sulphuric acid, gave 0·295 gr. of sulphate of barytes, or 0·194 barytes. The solution which contained the peroxide of uranium was evaporated to dryness, and the dry salt heated to decomposition in a platinum crucible; for this a strong and long continued heat was required to drive off the last portion of the sulphuric acid. The residue was protoxide of uranium, which weighed 1·121. When dissolved in nitric acid, no turbidness appeared indicating the presence of any sulphate of barytes: 1·343 uraniate of barytes had thus given 0·194 barytes, and 1·121 protoxide of uranium, making together 1·315. The loss, amounting to 0·028, must of course be the difference between the quantity of oxygen in the protoxide and peroxide of uranium; but 1·121 : 0·028 :: 100 : 2·5; consequently 100 protoxide of uranium, in order to become peroxide, must combine with 2·5 oxygen.

1·456 gr. uraniate of barytes of another preparation gave 0·364 sulphate of barytes (equivalent to 0·239 barytes), and 1·186 protoxide of uranium. The oxygen driven off in this experiment is 0·031 with 1·186 protoxide; this to 100 parts is equivalent to 2·61. The mean of 2·50 and 2·61 is 2·55; and 100 parts of peroxide of uranium ought, according to these experiments, to contain 5·96 oxygen, and 100 of the metal combine with 6·34 oxygen, in order to become peroxide.

Although the uraniate of barytes was formed in both cases in the same way, so that the liquid from which it was prepared contained always a quantity of uncombined ammonia; and although a quantity of muriate of barytes in both cases remained unprecipitated in the liquid, yet we perceive that the uraniate of barytes first prepared contained a considerably smaller proportion of basis than the second. It is possible that the peroxide of uranium, being a weak acid, may undergo some modification in its capacity of saturation, according as the muriate of barytes is present in a greater or smaller proportion relative to the muriate of uranium.

Analysis of the Sulphate of Uranium and Potash.

This double salt being less soluble in water than the muriate of uranium and potash, crystallizes more readily, and may, by means of crystallization, be completely freed from any excess of the salt of potash. I consider this salt on that account as more suitable for analysis than the muriate.

Potash-sulphate of uranium is obtained by evaporating a solution of sulphate of uranium to which some sulphate of potash has been added. It forms a confused crystallized mass of an exceedingly beautiful lemon-yellow colour. When heated, it gives out in the first place water, and when the heat is augmented melts, and flows into a liquid. The fused salt is greenish-yellow, and therefore has probably undergone a partial decomposition; although it gives a full lemon-yellow solution in water; but if the salt be heated only to a commencement of fusion, it retains its yellow colour completely.

A solution containing 2·172 grs. of crystallized and anhydrous potash-sulphate of uranium was precipitated by muriate of barytes. The sulphate of barytes being separated, washed, and heated to redness, weighed 1·814 gr. corresponding to 0·623 sulphuric acid.

The filtered liquid was saturated with caustic ammonia, which precipitated uraniate of barytes. It was collected on the filter and weighed. The residual liquid was mixed with a sufficient quantity of sulphuric acid both to separate any barytes that might remain, and in order finally to obtain a sulphate of potash quite free from muriatic acid. After this the whole liquid was evaporated to dryness; the dry salt was heated to redness in a weighed platinum crucible, by which the ammoniacal salts were volatilized, and sulphate of potash remained, which was rendered neutral by the fumes of carbonate of ammonia. It weighed 0·533 gr. equivalent to 0·288 gr. of potash. The salt dissolved in water without residue, and the solution was neither rendered turbid by ammonia, nor by nitrate of silver; showing that it contained neither oxide of uranium nor muriatic acid. The sulphuric acid and the potash were thus determined; what is wanting to make up the original quantity of salt employed must be peroxide of uranium. It follows that the constituents of the double salt must be:

Sulphuric acid	0·623
Potash	0·288
Peroxide of uranium	1·261
	2·172

Or in 100 parts:

Sulphuric acid	28·68
Potash	13·26
Peroxide of uranium	58·06
	100·00

13·26 potash are saturated by 11·26 sulphuric acid. There remain 17·42 acid which belong to the 58·06 of peroxide of uranium; but 17·42 sulphuric acid saturate a quantity of basis containing 3·477 oxygen; 100 parts of peroxide of uranium consequently contain 5·99 oxygen; or 100 parts of the metal, in order to be peroxidized, combine with 6·37 oxygen.

The oxygen of the potash is to that of the peroxide of uranium as 2 to 3; for the oxygen in 13·26 potash is 2·248, and in 58·06 of peroxide of uranium 3·477. Now 2·248 : 3·477 :: 100 : 154·7.

If we now collect the results of all these experiments to determine the quantity of the oxygen in the peroxide, we shall find them as follows:

100 parts of uranium take up, according to the analysis of

	Oxygen.
Uraniate of lead	5·559
Uraniate of barytes	6·340
Potash-sulphate of uranium.	6·370

The number 5·559 has almost the same ratio to the oxygen in the protoxide that 3 has to 2; for in the protoxide 100 parts of uranium are combined with 3·688 parts of oxygen, which, multiplied by 1½, = 5·532; but the last two numbers lie between 1½ and twice 3·688. It is difficult to determine which of these numbers come nearest the truth. The last two, although obtained different ways, accord in a remarkable degree; and have in consequence some claim to be considered as accurate. But on the other side it is clear that the experiments made with uraniate of lead ought to come out with greater precision; because the analytical method followed with respect to it puts it in our power to attain a higher degree of accuracy than is likely in the two following experiments. I must in the mean time acknowledge that these experiments do not furnish us with the knowledge of the composition of the oxides of uranium with that degree of accuracy which chemists have a right to require. At the same time it may be admitted as most likely that the oxygen in the peroxide of uranium is 1½ times as great as in the protoxide; unless we were to admit that the second composition, such as it was found, can be completely depended on; and this there is not much reason to do, as both the oxidizement of the metal and the reduction of the protoxide, give the same composition of the oxide.

According to M. Schonberg's experiments on the composition of the oxides of uranium,* the oxygen in the protoxide is to that in the peroxide as 2 to 3; for he found the protoxide to contain 6, and the peroxide 8·73 per cent. The oxygen in the protoxide was determined by analysing the protomuriate of uranium; and the composition of the peroxide was determined by the loss of weight which it sustained when heated to redness, by which it was converted into protoxide. But it appears from my experiments, that the protomuriate can never be obtained neutral and free from peroxide without undergoing decomposition. It is probable, therefore, that M. Schonberg's experiments were made upon a salt containing an excess of acid, in which case it is obvious that he must have overrated the quantity of oxygen contained in the basis. The peroxide employed in his second determination was obtained by decomposing pernitrate of uranium by means of heat; but this mode of experimenting cannot be decisive, as from what has been stated it must be evidently impossible to drive off the nitric acid completely from this salt without converting the basis partly into a protoxide.

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* De conjunctione chemica ejusque rationibus. Upsaliæ, 1812.

100 parts of uranium to form the protoxide combine with 3·688 oxygen. If this represents two atoms of oxygen, as is probable, then an atom of uranium will weigh 5422·99. Peroxide of uranium precipitated by a caustic alkali is insoluble in an excess of the precipitating substance, and always contains a little alkali, which cannot be washed out, but is found, together with an earthy or metallic salt in the water, as the peroxide of uranium is precipitated in combination with an earth or metallic oxide. In alkaline carbonates, and in particular with carbonate of ammonia, peroxide of uranium is easily soluble. The solution has a strong lemon colour, and even an inconsiderable portion of peroxide of uranium is capable of giving that colour to water. By boiling, the peroxide of uranium precipitates in the state of a pale-yellow powder, which contains carbonic acid, and even some ammonia. We must not, however, reckon upon obtaining the whole oxide; because however pure it may be, there always remains a little in the water; and when the carbonated oxide is put upon the filter and washed, the water employed in the edulcoration, especially towards the end, contains a good portion of oxide in solution, which again falls when the liquid comes to be mixed with the saline solution. The peroxide precipitated by caustic ammonia acts in precisely the same way. Peroxide of uranium, therefore, may be considered as slightly soluble in water. This solubility is partly, but not entirely prevented by washing it with water containing sal ammoniac dissolved in it. On the other hand, if peroxide of uranium be united to an earth or a metallic oxide, it is not in the least soluble in water.

According to what has been already stated, peroxide of uranium unites with the other earths and metallic oxides into a kind of uraniates, in all cases, when the mixed solutions of both are precipitated by caustic ammonia. These compounds may be reduced by means of hydrogen gas to the state of alloys of uranium, and the basis of the earth or oxide employed, even when the earthy basis is barytes. These alloys again absorb oxygen at the common temperature of the atmosphere, and the oxidizement is accompanied by combustion. They constitute, therefore, a peculiar kind of *pyrophori* not inferior to those already known. The alloy of uranium and lead has been already mentioned. An analogous body is obtained when uraniate of barytes is reduced; and the alloy of uranium and iron burns still better than either of the others.

Uranium appears to have a very weak affinity for sulphur. The sulphuret may be obtained by the moist way, as is already known, if a solution of uranium be precipitated by an alkaline hydrosulphuret; but I have not been able to succeed in forming sulphuret of uranium by the dry way. When dry sulphuretted hydrogen gas is passed over red-hot protoxide of uranium, the oxide is reduced immediately, water and sulphur exhale, and a body remains in all respects similar to the metallic uranium, and which contains only 1·61 per cent. of sulphur.

Salts of Uranium.

The protosalts of uranium in a state of purity are not easily prepared. If we dissolve protoxide of uranium in concentrated sulphuric or muriatic acid (which, in the second case, goes on very slowly), the solution at first is dark bottle-green; but it becomes gradually lighter, and at last assumes a greenish-yellow colour from the formation of peroxide. The sulphuric acid solution when evaporated deposits a light green confusedly crystallized mass which contains a mixture of protoxide and peroxide of uranium. The muriatic acid solution may be evaporated to dryness without the deposition of any crystals.

Persalts.—The persulphate of uranium is formed, when nitric acid is added to a solution of protosulphate of uranium. This addition, the green colour speedily passes to yellow, even without the assistance of heat.

The salt does not crystallize even when evaporated to the consistence of a syrup. If we continue the application of the heat after the salt has become dry, the oxide loses a portion of its oxygen, and the mass acquires a greenish-yellow colour.

Pernitrate of uranium is formed when the protoxide is dissolved in nitric acid. The solution goes on with rapidity, especially if assisted by heat. Nitrous gas is disengaged, and we obtain a yellow liquid, which, when evaporated, shoots into long prismatic crystals of a fine yellow colour. The salt dissolves readily in water, and is decomposed in a comparatively low temperature; oxygen gas is also disengaged, and pernitrate of uranium formed. When heated just to redness, it is decomposed completely; nitrous acid is driven off, and protoxide of uranium remains.

Permuriate of uranium is formed in the same way as the persulphate. It does not crystallize, though evaporated to the consistence of a syrup. The dry salt is very deliquescent.

Percarbonate of uranium has been already described.

Double Salts.

Peroxide of uranium seems to have a great disposition to form double salts in combination with other bases. It has been already stated that a solution of persulphate or permuriate of uranium does not crystallize; but if a portion of sulphate or muriate of potash be added to the solution, we obtain by evaporation a uranium salt in combination with the alkaline salt.

Potash-persulphate of uranium forms a granular crystallized mass, of a very fine lemon-yellow colour. The salt dissolves pretty easily in water. By alcohol it is decomposed in such a way that the persulphate of uranium is dissolved, and the alkaline salt remains behind undissolved. When heated, it loses its water, melts at a low red heat, and begins in this temperature to undergo decomposition; for it acquires a green colour; but the decomposition is very inconsiderable, for even after being completely fused, it dissolves again in water with a lemon-yellow colour. Before it begins to melt it is not altered in the least. The constituents of this salt in an anhydrous state have been given above.

Ammonia-persulphate of uranium crystallizes like the preceding salt. It is easily soluble in water, and in a higher temperature it is decomposed, leaving protoxide of uranium.

Potash-permuriate of uranium crystallizes likewise, if we add a considerable proportion of muriate of potash to the solution of peroxide of uranium in muriatic acid; but unless this be done, the double salt crystallizes very slowly. It forms small crystals, sometimes in prisms, sometimes in grains. They are transparent and yellow, and have a regular form; but are mechanically mixed with muriate of potash; from which however they may be picked out quite pure; but the process is very tedious, as the crystals are small. The mixed muriate of potash cannot be separated by crystallizing again, for as it is just as soluble in water as the double salt, they both crystallize at the same time. When heated, the double salt gives out water without being decomposed. It melts when it begins to be red-hot, giving out chlorine gas. It becomes green, and is decomposed, though only partially.

I have not examined any other double salts, though it is likely that more of them might be easily obtained.

ARTICLE V.

Examination of the Oxidum Manganoso-Manganicum, a hitherto unknown Chemical Compound of the Protoxide and Deutoxide of Manganese. By Aug. Arfwedson.*

In consequence of the experiments which have been made upon the oxides of manganese, it has been concluded that the products formed, when protoxide of manganese is heated to redness in the open fire, and when nitrate of manganese is exposed to a red heat, constitute one and the same oxide; and that the different colours (for the former is brown and the latter black) are owing to the different states of mechanical division in which the two bodies are. But I have always obtained these two products, very different from each other in appearance, even when both were reduced to powders of the same fineness. The former always gives with sulphuric acid a weak amethyst-red solution; while the latter forms with the same acid a solution having a deep grass-green colour. These different properties appeared to require a more accurate investigation of the chemical constitution of these two bodies. With this view I have, at the desire of Prof. Berzelius, made a set of experiments, of which I shall now give a short account.

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* Translated from the Afhandlingar i Fysik, Kemi, [illegible]

(1.) A neutral solution of pure muriate of manganese was precipitated by bicarbonate of potash. The carbonate of manganese obtained was snow-white. After being washed with boiled and cold water, and dried in the vacuum of an air-pump with sulphuric acid, it remained without any change in its colour.

The carbonate of manganese thus prepared was put into a spherical cavity blown in the middle of a barometer tube, through which a current of hydrogen gas was made to pass. As soon as the gas had driven out all the atmospherical air, the manganese was heated by applying a spirit-lamp to the glass ball containing it; and the heat was continued as long as any water and carbonic acid gas continued to be evolved. The powder by this process acquired a fine pistachio-green colour, which continued till the apparatus had cooled, and the atmospherical air was admitted to it, when it became greyish-green. A little portion of this powder treated with muriatic acid dissolved without the least effervescence, showing that the carbonic acid had been completely expelled.

A gramme of protoxide prepared in this way was heated in a glass capsule over a spirit-lamp. The matter immediately took fire, and burnt to a dark-brown oxide, which weighed 1·0735 gramme. Thus the increase of weight on 100 parts of protoxide was 7·35; and as the protoxide is considered as containing 21·9 per cent. of oxygen, the brown oxide must be a compound of

Manganese	72·784	100·00
Oxygen	27·216	37·39
	100·000	137·39

The 1·0735 gramme of brown oxide was boiled with nitric acid to dryness, and heated to redness. The brown colour of the oxide gradually changed to black, with a copious evolution of nitrous gas, and the weight of the powder became 1·195 gramme. Exposed to a stronger heat in a platinum crucible over a charcoal fire, the weight was reduced to 1·100 gramme. Thus 100 parts of protoxide by this treatment increase 10 parts in weight; and according to the preceding statement of the constitution of the protoxide, this black oxide is composed of

Manganese	71	100·00
Oxygen	29	40·84
	100	140·84

(2.) 1·192 gramme of protoxide of manganese, prepared as before described, left, when burnt in the open fire, 1·383 gramme of brown oxide. According to this experiment 100 parts of protoxide took up 7·043 oxygen, and the constituents of brown oxide are:

Metal	100·00
Oxygen	37·45
	137·45

The 1·383 gramme of brown oxide was boiled to dryness with nitric acid, and exposed to a strong red heat in a platinum vessel. The matter had acquired a shining black colour, and weighed 1·398 gramme. The increase of weight this time was so inconsiderable that it seems probable that some disoxygenation had taken place. It may be presumed, therefore, that the black oxide when exposed to a violent heat may be completely reduced to the state of brown oxide. The heat was continued for nearly an hour, and was as great as could be applied. The powder now resumed its former brown colour, and very nearly its original weight. The excess amounted only to two millegrammes.

To ascertain whether the black oxide can be prepared by means of a smaller determinate degree of heat, so as to give constant results, the oxide of manganese already heated to redness was again treated with nitric acid, and heated over a spirit-lamp five different times, and the increase of weight was noted each time. The following little table shows the result.

100 parts of protoxide had taken up

1st time	14·4 parts of oxygen.
2d	13·93
3d	10·52
4th	10·37
5th	10·13

The smaller augmentation of weight in the last three experiments was owing to a further evolution of nitrous gas. The experiments show that the true increase of weight lies between 10·52 and 10·13. We may, therefore, consider the increase of weight of 100 parts of protoxide, when converted into black oxide, to be 10·4; so that black oxide is composed of

Manganese	100·00
Oxygen	41·35

I conceive that I may conclude from these experiments, that we always find the weight of oxygen in the black oxide of manganese prepared by means of nitric acid, a circumstance which is often of considerable importance in the analysis of minerals, in which the question often is to determine the composition of a given quantity of oxide. Perhaps the best method of proceeding may be to expose the oxide to a strong red heat till it is converted into brown oxide, the composition of which is always uniform, and of course the quantity of oxygen in it may be known with great accuracy.

According to the preceding experiments, the quantity of oxygen in the protoxide is to that in the black oxide as 1 to 1½; for 100 parts of the metal combine in the protoxide with 28·107 parts of oxygen, and this quantity multiplied by 1½ makes 42·160. The brown oxide, on the other hand, appears from the quantity of its oxygen to be intermediate between the protoxide and black oxide; but the oxygen constitutes no multiple either of the oxygen in the protoxide or black oxide. It must, therefore, be a combination of these two oxides. If this compound, like the oxydum ferroso-ferricum be supposed so constituted that the oxide contains twice as much metal, and three times as much oxygen, as the protoxide, then it will be a compound of

Manganese	72·82
Oxygen	27·18
	100·00

Direct experiments have given us

Manganese	72·784
Oxygen	27·216
	100·000

numbers which correspond as nearly with the calculated quantities as could be expected. On the other hand, we find that this compound by calculation may be composed of

Black oxide of manganese	68·932
Protoxide of manganese	31·068
	100·000

I have called this oxide oxidum manganoso-manganicum, on account of its resemblance to the oxidum ferroso-ferricum, the constitution of which has been explained by Prof. Berzelius in his Attempt to lay the Foundation of a new scientific System of Mineralogy, p. 92.

It remains now to ascertain how far the so called *grey ore of manganese* and the crystallized varieties of that mineral usually consist of manganese in the form of superoxide, or if they may not consist of black oxide, which we have seen gives out oxygen gas also when heated to redness.

1. *Crystallized Grey Manganmalm, from Udenäs, in West Gothland.*

5·035 grammes of the pure crystallized ore was heated over a spirit-lamp in a small retort, to the beak of which was luted a glass tube filled with dry chloride of calcium. The matter, as soon as the heat was applied, gave out much water, which by degrees passed over, and was absorbed by the chloride. The heat was continued as long as any moisture was perceptible in the beak of the retort. The apparatus being now allowed to cool was weighed, and the loss of weight amounted only to two millegrammes, a loss so small that it may be overlooked. Thus no gas had been extricated. The mineral by this exposure to heat had lost 0·508 gramme, which may, therefore, be reckoned the weight of the water contained in the specimen. The residue amounted to 4·527 grammes; of this 4·504 grammes were exposed to a strong red heat in a platinum crucible: the loss of weight amounted only to 0·022 gramme. Being exposed a second time to as high a temperature as the fire could give, the loss of weight was increased by 0·139 gramme. A third heating diminished the weight by 0·016. When heated for the fourth time no further loss of weight was sustained. The crystals still retained their shape; but the colour had been changed from black to reddish-brown, and the powder had a cinnamon-brown colour exactly similar to the above described oxidum manganoso-manganicum. The whole loss of weight amounted to 0·177 gramme. This, though only from 4·504 grammes, may be taken without sensible error for the whole loss from 4·527 grammes, as the error only begins to be perceptible in the fourth decimal place.

Thus the mineral had given

Water....................	0·508 or	10·08
Oxygen....................	0·177	3·51
Oxidum mang.-manganicum.	4·350	86·41
	5·035	100·00

The small quantity of oxygen gas obtained shows evidently that the mineral under examination cannot be a superoxide of manganese; and it seems plain that the manganese is in the state of black oxide; for we find by an easy calculation, that the 86·41 parts of oxidum manganoso-manganicum obtained require very nearly 3·51 parts of oxygen to be converted into black oxide. The constituents by calculation in 100 parts are,

Oxidum manganoso-manganicum....	86·85
Oxygen	3·07
Water.	10·08
	100·00

Now the 86·85 parts of the oxidum manganoso-manganicum contain 23·6 parts, and 10·08 parts of water contain 8·895 parts of oxygen. Thus it appears that the oxygen in the oxide which has been heated to redness is not a multiple of the oxygen in the water; but if we add the 3·07 parts of oxygen driven off by the red heat to the 23·6 which are found in that oxide, then the unheated mineral will be a chemical compound with water, whose oxygen is just one-third of the oxygen in the oxide. The hydrogen of the water driven off and its oxygen, added to the oxide, make up the constituents of the superoxide.

It is plain from the preceding experiments that the mineral just examined is a hydrate of manganese-oxide, whose oxygen contains three times as much oxygen as that in the water. The formula for this compound is $\dddot{Mn}$ Aq.

II. *Common Grey Ore of Manganese.*

a. 5·03 grammes of this mineral in powder freed from water mechanically lodged in it were put into a small glass retort, to the beak of which was luted a glass tube filled with dry chloride of calcium. The retort was heated over a spirit-lamp as long as any moisture was disengaged. The apparatus being allowed to cool was found to have lost 0·143 gramme of its weight; this was gas disengaged. The chloride of calcium had increased in weight 0·077 gramme, which was water. The residue weighed 4·810 grammes.

b. Three grammes of the powder thus freed from water and a portion of gas were exposed to a strong red heat in a platinum crucible. After three repetitions of this process, the loss of weight was 0·286 gramme; and the blackish-grey colour of the mineral was changed into brown. A fourth exposure to heat was tried without any diminution of weight. Since 3 grammes by this treatment lost 0·286, it is evident that if the whole quantity had been thus treated, it would have lost 0·458 gramme. If to this we add the 0·143 lost over the spirit-lamp, we see that the 5·03 grammes of the mineral had lost 0·601 gramme of oxygen and 0·077 of water.

c. The 3 grammes employed in the preceding experiment were dissolved in muriatic acid. The solution was evaporated to dryness, and the dry mass again dissolved in water acidulated with muriatic acid. There remained undissolved silica and earthy matter, which, after being washed and heated to redness, weighed 0·442 gramme. The filtered solution was digested with an excess of caustic potash; the precipitate formed was collected on the filter and washed. The alkaline ley which passed through was examined by the usual reagents, but it was not found to contain any thing in solution. The precipitate by the caustic potash was dissolved from the filter in muriatic acid. The solution, after being neutralized by ammonia, was mixed with some drops of oxalate of ammonia, but the liquid continued clear. It was afterwards mixed with succinate of ammonia as long as any precipitate fell. The succinate of iron thus obtained was separated and washed. Being heated to redness the residual oxide of iron weighed 0·124 gramme.

As this 0·124 gramme of oxide of iron, together with the 0·442 gramme of silica and matrix, must be separated from the 3 grammes of the mineral examined, the residual pure oxide of manganese amounts to 2·434 grammes; and consequently the 4·81 grammes of the heated mineral in *a* contained 3·902 grammes of pure oxide. If to this we add the 0·22 gramme of gas and water driven off in *a*, the whole quantity of the mineral freed from foreign matter was 4·122 grammes, and its constituents were,

Oxygen, *a*	0·143		
b	0·458		
	——	0·601	...... 14·58
Water, *a*		0·077	...... 1·86
Oxidum man.-mangan.		3·444	...... 83·56
		4·122	100·00

The small quantity of water may be considered as mechanically lodged in the mineral. 83·56 parts of oxidum manganoso-manganicum contain 22·71 parts of oxygen. Thus the remaining 60·85 parts of manganese had been united with 22·71 + 14·58, or 37·29 parts of oxygen, which for 100 parts amounts to 61·45.

The superoxide of manganese is considered as a compound of 100 metal and 56·213 oxygen. The oxygen found in the mineral, therefore, is too high; but this is probably owing to some error in the analysis, the principal object of which was not to determine the constitution of the superoxide, but only to ascertain if the mineral was in this higher state of oxidizement; and the result shows clearly that the manganese was in the state of a superoxide.

Mineral superoxide of manganese is easily distinguished from the hydrate by the different colours of their powders. The powder of the superoxide is a full black, while that of the hydrate on the other hand is yellowish-brown. We have only to scrape the minerals with a knife; the difference is immediately seen.

Protoxide of Manganese.

A globular cavity was blown in the middle of a barometer tube, which was filled with pure carbonate of manganese, and a current of muriatic acid gas was passed through it. The extrication of gas took place without any evolution of heat till the salt appeared completely decomposed. The salt was now heated by a spirit-lamp at first slowly, but at last the greatest possible degree of heat was given. The muriate thus formed was rose-red, and it retained its colour, though the muriatic acid gas driven over was at last mixed with hydrogen gas. The salt thus obtained weighed 1·52 gramme. Being dissolved in water there remained behind 0·012 gramme of oxide of manganese. The remaining 1·508 was a neutral salt. The solution was quite colourless. The muriatic acid was thrown down by nitrate of silver, and gave, after the usual treatment, 3·408 grammes of horn silver, which contains 0·650 gramme of muriatic acid. This acid was combined with 0·858 gramme of protoxide of manganese; but 0·65 muriatic acid saturate a quantity of base, whose oxygen amounts to 0·191; consequently the 0·858 gramme of protoxide and manganese contain this quantity of oxygen. The experiment gives us 100 parts composed of

Manganese	77·856	100·00
Oxygen	22·144	28·44
	100·000	128·44

John, by direct analysis of neutral sulphate of manganese, found the protoxide of this metal to contain 21·875 per cent. of oxygen. The oxygen, according to my experiment, is a little more; but I have reason to conclude that a small quantity of black oxide existed in the muriate of manganese which I examined. The result of my experiment, therefore, may, perhaps, be the less to be depended on.

Appendix.

That the English reader may fully understand the preceding paper, a few observations may be necessary.

1. The atomic weight of manganese is 3·5, and the atomic weight and constituents of its oxides are as follows:

		Composition.	
	Atomic weight.	Metal.	Oxygen.
1. Suboxide	4·0	3·5	+ 0·5
2. Protoxide	4·5	3·5	+ 1·0
3. Deutoxide	5·0	3·5	+ 1·5
4. Tritoxide	5·5	3·5	+ 2·0
5. Manganesous acid	6·5	3·5	+ 3·0
6. Manganesic acid	7·5	3·5	+ 4·0

2. The oxidum manganoso-manganicum of Arfwedson is a compound of 1 atom protoxide and 2 atoms deutoxide.

1 atom protoxide	4·5
2 atoms deutoxide	10·0
	3)14·5
	$4·83\dot{3}$

so that its atomic weight (reduced to the lowest terms) is $4·83\dot{3}$.

3. According to Berzelius the atomic weight of manganese is 7·1157, and the names and constituents of the different oxides are, according to him, as follows:

	Atomic weight.	Composition. Metal.	Composition. Oxygen.
2. Oxydule	9·1157	7·1157 +	2
3. Oxide	10·1157	7·1157 +	3
4. Superoxide	10·1157	7·1157 +	4

4. Berzelius represents these three oxides by the following symbols:

1. Protoxide $\overset{\cdot\cdot}{\mathrm{Mn}}$.

2. Deutoxide $\overset{\cdot\cdot\cdot}{\mathrm{Mn}}$.

3. Tritoxide $\overset{\cdot\cdot\cdot\cdot}{\mathrm{Mn}}$.

denoting the number of atoms of oxygen by the dots above the letters Mn.

ARTICLE VI.

Account of a new Mineral Substance. By M. Levy, MA. in the University of Paris.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *March* 12, 1824.

UPON a specimen from Arendal, belonging to Mr. Turner's collection, I have lately observed with cleavelandite,* flesh-coloured felspar, and green amphibole, some small brilliant black crystals, the description of which I now send you, because I believe they belong to a new mineral species. Their form is

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* The substance I call here cleavelandite forms the greatest part of the specimen; it has much the appearance of cleavelandite; but however I have not been able under the small particles I have detached to obtain cleavage sufficiently brilliant for measurement.

Fig. 1.

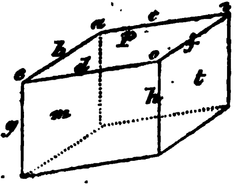
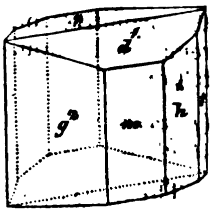


Fig. 2.



represented by fig. 2; in some of them, however, the plane *m* and its opposite are wanting, so that the prism is then six-sided instead of being eight-sided. These crystals cleave easily with brilliant surfaces parallel to the planes *p* and *t*, fig. 2. There is also an indication of cleavage parallel to the plane *m*. All the natural planes, as well as those obtained by cleavage, are sufficiently brilliant to allow the use of the reflective goniometer for the measurement of their incidences. From the measurements I have taken, and the cleavages already mentioned, I am induced to take for the primitive form of this substance a doubly oblique prism, fig. 1, in which the incidences of *p* on *m* and *t* are respectively 92° 34′ and 88°, that of *m* on *t* 112° 30′, and in which the three edges *d*, *f*, *h*, which meet at the solid angle *o*, are to each other nearly as the numbers 13, 20, 11. The incidences of *p* on *m* and *t* are nearly supplement of each other, the only difference being 34′, hence the primitive form differs but little from an oblique rhombic prism; for if these two angles were exactly supplement of each other, then the incidences of *p* on *m*, and on the face behind parallel to *t*, would be equal, and consequently the primitive would, at least so far, have the character of an oblique rhombic prism. There is another incidence which might, without a proper attention, lead to the same conclusion respecting the nature of the primitive. It is the incidence of the planes h^1 and g^2, fig. 2, which is equal to 89° 20′; that is to say, very near a right angle. Now if the planes g^2 and h^1 were considered as the diagonal planes of a rhombic prism, they should be perpendicular to each other. These indications of an oblique rhombic prism, as the primitive form of this substance, are, however, carried no further, and are entirely destroyed by the want of the symmetry which should accompany them. The faces h^1, g^2, if the diagonal planes of a rhombic prism, should be equally inclined upon the two lateral planes which they meet; and here we find that the incidences of h^1, with the planes *m* and *t*, as well as those of g^2 with *m*, and the plane parallel to *t*, differ widely from each other. The occurrence of the plane d^1 without being accompanied by a plane replacing the edge of intersection of plane *p* with the plane parallel to *t*, is also incompatible with an oblique rhombic prism. No doubt can remain, therefore, as to the primitive form being a doubly oblique prism. I have thought it would not be useless to place here the discussion of the observations which might lead to assume another form as the primitive, on account of the ambiguous characters of this remarkable form. The incidences of the planes of fig. 2 are as follow:

$(p, m) = 92° 34'$ $(p, t) = 88°$ $(m, t) = 112° 30'$ $(m, h^1) = 137° 5'$
$(t, h^1) = 155° 25'$ $(p, d^1) = 150° 25'$ $(g^2, m) = 132° 15'$.

These crystals scratch glass easily. This substance I propose to call *Babingtonite*, in honour of the late President, and one of the founders of the Geological Society of London. His claims to have his name thus recorded in mineralogy, are too many, and too well known to every well-wisher of this science, to require any comment by me.

In Mr. Turner's catalogue, I had given the same name to a substance from Freyberg; but I find that Mr. W. Phillips, in his last work on Mineralogy, has noticed the same substance, and designated it under the name of sulphuret of silver and antimony, which name there is not the least ground to change.

Mr. Children has kindly undertaken to examine with the blowpipe a small quantity of babingtonite.

ARTICLE VII.

Examination of Babingtonite by the Blowpipe.
By J. G. Children, Esq. FRS. &c.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *March* 14, 1824.

IN *glass matrass*, the Babingtonite decrepitates *very slightly*, and gives off a dense vapour, which soon disappears. A thin film of pure water condensed on the sides of the tube. Appearance of the assay not altered.

Alone, in forceps, fuses on the surface, pretty readily, into a black enamel.

With soda, on platina wire, in the *oxidating flame*, the assay gives a dark-green opaque globule; the addition of nitre heightens the colour.

In the *reducing flame*, the colour changes to dark-brown, or nearly black; globule opaque.

With borax. P. W. In O. F. deep amethyst-coloured globule; in R. F. colour changes to bluish-green; globule perfectly transparent in both flames.

With salt of phosphorus. P. W. In O. F. scarcely any action on a minute fragment of the assay; globule transparent, orange-yellow, while hot; when cold, colourless. In R. F. the same, but colour greenish while hot.

With the *same flux*, and the assay in fine powder; in O. F. solution more easy; but a considerable silica skeleton remains undissolved: colour as before, but deeper. In R. F. nearly colourless, hot; when cold, slightly inclining to an amethystine colour. By the addition of a morsel of tin foil, the amethyst colour a very little deeper.

With *nitrate of cobalt*, black mass, without any indication of alumina.

In addition to the silica, iron, and manganese, clearly indicated by the preceding experiments, I obtained, *via humida*, a considerable proportion of lime. By the action of the salt of phosphorus, in the reducing flame, there appears to be a minute portion of titanium present, but want of time, and a larger quantity of the assay, prevented my obtaining any very decisive results in that respect by operating in the moist way: though they tended to strengthen the probability of its being contained in the mineral: its quantity however must be very minute.

Article VIII.

Astronomical Observations, 1824.

By Col. Beaufoy, FRS.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR,

I SHALL be much obliged to any of your astronomical correspondents, if they will favour me with their observations on the eclipse of Jupiter's third satellite, which occurred on the 26th of last January, as I think it is probable I committed an error by mistaking one satellite for another, in the observations published in the *Annals* for March.

I remain, dear Sir, yours very truly,
MARK BEAUFOY.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Feb.	21.	Emersion of Jupiter's first satellite	9h	39′	02″	Mean Time at Bushey.
			9	40	23	Mean Time at Greenwich.
March	3.	Emersion of Jupiter's second satellite	11	08	36	Mean Time at Bushey.
			11	09	57	Mean Time at Greenwich.
March	8.	Emersion of Jupiter's first satellite	7	58	22	Mean Time at Bushey.
			7	9	43	Mean Time at Greenwich.
March	12.	Occultation of ε Leonis by the moon. Immersion	7	23	34·7	Mean Time at Bushey.

ARTICLE IX.

Mean Results, &c. of various Meteorological Registers for the Year 1823.

I. By Mr. M. P. Moyle, at Helston, Cornwall.

1823.	Barom.*	Self-Registering Therm. In the shade.	Self-Registering Therm. In the sun.	Com. ditto. In the shade.	Hygrometer.			Rain Guage.			Wind and Weather.		
	Monthly mean from the daily means.	Monthly mean.	Monthly mean.	Monthly mean.	Max.	Min.	Mean.	No. 1. 30 Feet.	No. 2. 5 Feet.	Mean.	Prevailing.	Rainy and showery.	Dry days.
January	29·6153	37·97	40·96	38·59	—	—	—	6·82	—	6·820	NE	20	11
February	29·5182	41·61	45·14	42·37	—	—	—	4·83	—	4·830	W & SW	22	6
March	29·8719	44·63	48·37	45·27	—	—	—	2·52	—	2·520	NW	20	11
April	29·8454	46·43	50·45	47·37	—	—	—	1·45	1·60	1·525	E	12	18
May	29·8848	54·58	59·63	55·04	—	—	—	1·96	2·08	2·025	SW	15	16
June	29·9149	55·26	62·10	55·84	100	49	78·79	1·69	1·94	1·815	E & SW	14	16
July	29·8606	54·95	66·69	57·97	99	55	83·88	2·72	2·94	2·835	W	18	13
August	29·8855	50·43	67·07	58·85	100	49	85·60	6·12	6·20	6·160	SW	20	11
September	29·6010	59·91	65·11	56·29	100	51	81·16	3·55	3·63	3·590	NW	14	16
October	29·7006	49·88	55·82	51·38	99	52	84·15	4·70	4·80	4·750	W	15	16
November	30·1828	47·60	52·13	47·50	—	—	—	3·68	3·98	3·850	E	10	20
December	29·8268	45·06	48·09	45·86	—	—	—	5·12	5·67	5·395	SW	20	11
Annual means, &c.	29·8094	49·79	55·20	50·194	—		Total	45·25		46·125		200	165

The Pluviameter, No. 1, is about 30 feet above the ground, and 135 feet above the level of the sea; and No. 2, about 5 feet above the ground.

* Barometer 105 feet above sea level. Corrected to 32° of Fahr.

Jan.—The snow continued 12 days, which is much longer than usual.

Feb.—A very wet month.

March 4 and 5, very stormy. The gusts of wind were extremely sudden, with wind to the NW.

April.—On the morning of the 5th, the wind changed suddenly from the SW to NE, and continued very boisterous till noon: 26. Heavy hail shower, with distant thunder.

May.—A cold month, except the few last days, which were remarkably fine.

June.—Very cold. Fires required by many in the evenings: some thunder.

July.—Remarkably cold, wet, and boisterous; scarcely two days fine successively.

August.—A wet month; much thunder and lightning on the 22d and 24th; about two inches of rain fell in the morning of the 24th in the course of six hours.

Oct.—The wind rose suddenly in the evening of the 30th to a perfect hurricane, which continued throughout the night. The barometer fell about 1 inch in about 24 hours.

Dec. 29.—Much lightning.

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2. By Mr. Edward Collins Giddy, at Penzance, Cornwall.

1823.	Barometer and Attached Thermometer.				Night and Day Ther.	Common Therm.
	Mean of the month.	True mean of the month.	Attached therm. max.	Attached therm. min.	Mean of the month.	Mean of month.
January ...	29·435	29·396	52	38	39·0	40·0
February...	29·355	29·311	52	42	43·0	43·2
March.....	29·715	29·665	52	45	46·0	45·1
April.....	29·700	29·638	56	48	47·0	47·2
May	29·740	29·662	62	54	55·0	56·2
June......	29·765	29·682	62	56	55·5	57·1
July	29·710	29·621	64	58	59·0	59·1
August....	29·745	29·652	64	60	59·5	60·2
September .	29·840	29·750	66	56	57·5	58·0
October....	29·540	29·468	62	48	51·5	53·0
November..	29·900	29·841	58	48	48·5	49·0
December..	29·645	29·589	58	48	46·5	47·0
Annual means, &c.	29·670	29·606	66	38	50·0	51·0
Ditto 1822	29·760	29·694	72	40	52·5	53·6

Barometer, 1823. Highest, Dec. 7, Wind, NW. 30·38; Lowest, Feb. 2, Wind, NE. 28·45.—Day and Night Thermometer. Highest, May 29 and July 20, Wind, S. 70°; Lowest, Jan. 16, Wind, NE. 27°.

3. By Col. Beaufoy, at Bushey Heath.

1833.	Rain.	Evaporat.
	Inches.	Inches.
January....	0·976	0·910
February....	2·537	1·360
March.....	0·758	3·020
April......	1·950	3·300
May June }	3·695	9·610
July	3·429	3·340
August	2·450	3·330
September. .	2·509	3·790
October	3·076	1·890
November ..	1·644	1·200
December ..	2·258	1·280
Total	25·282	32·430

4. By Mr. James Stockton, at New Malton, Yorkshire.

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1833.	Barom. Mean.	Ther. Mean.
January....	29·574	31·887°
February...	29·184	35·785
March.....	29·706	40·209
April......	29·856	44·033
May.......	29·895	54·419
June	29·920	54·916
July.......	29·711	58·774
August....	29·780	57·645
September. .	29·868	52·516
October....	29·680	45·741
November..	30·082	42·750
December..	29·514	36·710
Annual means, &c.	29·732	46·282

ANNUAL RESULTS.

Barometer.

	Inches.
Highest observation, Nov. 10. Wind NE.	30·880
Lowest observation, Oct. 11. Wind SE.	28·350
Range of the mercury	2·530
Mean annual barometrical pressure	29·732
Greatest range of the mercury in October.	2·180
Least range of the mercury in July.	0·900
Mean annual range of the mercury.	1·597
Spaces described by the mercury	93·180
Total number of changes in the year	154·000

Six's Thermometer.

Greatest observation, May 7. Wind, variable	77·000°
Least observation, Jan. 18. Wind, NW	9·000
Range of the mercury in the thermometer	68·000
Mean annual temperature	46·282
Greatest range in May	38·000
Least range in February	22·000
Mean annual range	29·918

Winds.

	Days.
North and East	51·000
North-east and South-east	79·000
South and West	98·000
South-west and North-west	112·000
Variable	25·000

Rain.

	Inches.
Greatest quantity, in February	7·040
Least quantity, in April	0·940
Total amount for the year	42·400

Observations.

Pressure.—The mean annual barometrical pressure (notwithstanding the extraordinary wetness of the period, is greater than that for many years past.

Temperature.—The mean annual temperature fully confirms what has been before advanced, that wet summers are generally cold; the whole of the monthly means, with the exception of May and December, are unusually low. Indeed the actual deficiency as to the annual amount exceeds 2½ degrees.

Winds.—These nearly agree with their respective numbers in 1822, and what is more strikingly remarkable, those of the SW exactly correspond.

Rain.—As to rain and snow, the amount is nearly unprecedented, and for the last three years it has been rapidly increasing.

ARTICLE X.

On the Effects of transmitting the Electrical through other Fluids.

By Mr. C. Woodward.

(To the Editor of the *Annals of Philosophy*.)

SIR, *March 12, 1824.*

As every subject connected with electrical science is daily assuming a more important feature, the following experiments and observations may not be uninteresting to some of your readers.

Place a piece of glass on the table of the universal discharger, and bring the pointed wires nearly in contact upon the surface of the glass; over the intersection formed by the wires, strew some loose gunpowder, and pass through it the charge of a jar containing about a square foot of coated surface, when it will be found that the powder will be invariably dispersed without inflammation.

Take a glass tube six or eight inches long, and about a quarter of an inch in diameter, fill it with water, and insert a cork at each end; through the corks pass pieces of wire so as to form a conducting communication with the water; place some loose gunpowder on the glass of the universal discharger as before, insulate the tube, and let it form part of the circuit; pass the charge through the water, and the gunpowder will be inflamed.

The small degree of intensity of charge required to produce the inflammation of gunpowder, when transmitted through a tube of water, is surprising; as the discharge of a quart jar indicating only an intensity of from 10 to 15 degrees is generally sufficient, and there appears to be little or no difference in the effects of tubes varying from 3 to 18 inches in length.

In prosecuting my inquiries to ascertain the cause of this singular effect, I found that the charge of a jar, which, when transmitted by good conductors, was sufficient to produce the fusion of 12 inches of iron wire, did not affect a single inch of the same wire, when passed through the tube of water; from which I concluded, that the intervention of the water tube, must have produced or prevented some mechanical effect.

I then pasted on a board, about three feet long, a narrow slip of tin foil, in which, at equal distances, four intersections, about one-eighth of an inch each, were made. I insulated the board, and placed over one of the intersections some loose gunpowder, and over each of the others six or eight wafers. On transmitting in the common way a charge through the tin foil, the powder was scattered, and the wafers blown three or four feet from the surface of the board; but on repeating the experiment with a tube of water, the powder was inflamed, and the wafers remained without any perceptible motion. This experiment led me to suppose, that a sufficient degree of heat under each circumstance was produced by the electrical fluid to inflame the powder; but when it passes through good conductors, the air is so suddenly and violently expanded, that the powder is scattered before it can be inflamed, as is the case with some of the grains when a musket is let off (for if it be fired over snow, a considerable portion of unconsumed powder will be perceived), but water being a less perfect conductor than the metals, by opposing a resistance to the passage of the electrical fluid, *retards its velocity*, and thus prevents the sudden expansion of the air, which would have scattered the powder before inflammation could have taken place.

This theory explains the circumstance of the charge having no effect upon the wires when transmitted through a tube of water. It is well known that intensity of charge is of the greatest importance in the fusion of wires. Now as intensity is velocity, it must follow, if the velocity of a charge be retarded, the intensity must be diminished; and hence no experiment requiring intensity of charge can be performed when the water tube forms part of the circuit.

If the tube be filled with other fluids, it will be found that the nearer the transmitting medium approaches to a good conductor, the greater will be the expansive effects; and as the expansive effects increase, the difficulty of inflaming the powder will increase also.

If the tube be filled with ether or alcohol, and placed in the circuit, the powder will be inflamed. If it be filled with sulphuric acid, which is a better conductor, the powder will be scattered and not inflamed, but the dispersion will not be so great as when metals only form the circuit. The same effect will be produced by transmitting the charge through the animal economy, or through water *not* inclosed in tubes, in which case the water does not appear to oppose a sufficient resistance to the passage of the fluid.

But the most remarkable circumstance attending these phænomena is, that it is immaterial whether the tube form part of the circuit *before* the electrical fluid passes the gunpowder, or *afterwards*. If the tube be placed on what I will call, for the sake of distinction, the negative side of the powder, either in immediate contact with the powder, the coating of the jar, or in any part of an interval in 20 yards of chain, the powder will be invariably inflamed when the charge is passed.

In this experiment the effect appears to precede the cause; and it may be asked, does it not prove the existence of two fluids? Some would no doubt answer in the affirmative, but so many difficulties attend the introduction of two fluids, and so many experiments militate against it, that I would refer to some other cause for an explanation of the fact.

Every substance which may be used as a conductor possesses a certain quantity of electricity, which, unless disturbed, remains in a dormant or a latent state. When a charge is passed through conductors, I conceive their natural quantity of electricity is driven off in the same way as water forced through tubes filled with the same fluid expels the water contained in them before any more can enter; and if an impediment were placed in *any part* of such tubes, the obstruction would be immediately felt throughout. So with the electrical fluid; the obstruction made by the introduction of water, which is a less perfect conductor than the metals, instantaneously affects the whole line of the circuit in whatever situation the tube may be placed; and by retarding the velocity with which the electrical fluid moves, prevents the expansive effects which the charge would otherwise have produced.

I am, Sir, your obedient servant,

CHARLES WOODWARD.

ARTICLE XI.

Hints to an Edinburgh Reviewer. By W. Phillips, FLS. &c.

IN the Edinburgh Review for January last, the concluding article is headed by the title page of the third edition of my Elementary Introduction to Mineralogy; but the "running title" is, with more propriety, "Mineralogical Systems." At the conclusion of the article, however, the Reviewer has bestowed on my little book half a page; and though not acquiescing in the correctness of all his observations, I have nothing to object against the tone and manner of the criticism.

But "there is a circumstance which merits attention, as it affects the degree of confidence which we can place in crystallographical indications" (p. 495), as reported by the Reviewer, who proceeds thus: "Similar *cleavage planes* in different individuals of the same species, meet in some cases under angles of different values. These differences are stated *by the author of the work before us,* as amounting even to forty minutes of a degree."

Now we might expect that a Reviewer should possess the several qualifications essential to the elevated position he voluntarily occupies, when he undertakes the task of criticising the works of others. First, that he should understand the subject; secondly, that he should read the work he criticises; and thirdly, that he should quote faithfully and accurately; but really I am compelled, though with regret, to conclude, that the Reviewer in question must be deficient in all these requisites, and he must excuse me if I proceed to the proof.

Supposing that the "author of the work before *us*," as the Reviewer has it, had not known better than to assert that the "cleavage planes of different individuals of the same species meet in some cases under angles of different values;" the Reviewer ought himself to have known enough of the subject to have enabled him to contradict the assertion, and to set the author and his own readers right, instead of building upon so unfounded a position, as the Reviewer has done, a sort of battery against "crystallographical indications;" which, indeed, as the case stands, is a very harmless one. But the Reviewer could not have been aware of the fact, that the *planes of cleavage* of different individuals of the same species *do always meet at the same angles*, if the cleavages and the selection of the minerals be made with proper care; and I cannot refrain from advising him to convince himself of this fact, by beginning with calcareous spar, sulphate of barytes, or any other of the several minerals which are commonly chosen for the first attempts of the tyro in this important and interesting department of the science. Let him give only an hour or two to cleavage and the reflective goniometer, and he will not fail to convince himself that if I had said what he attributes to me, I should have mis-stated a fact; but as he must be unacquainted with that well-known fact, I think it is sufficiently apparent that he must be deficient in the first requisite—a knowledge of the subject; that is, of mineralogy.

The existence of the second and third requisites may be discussed together. The place in which I have mentioned an occasional variation of 40 minutes in angles obtained by the reflective goniometer, and I believe the only place, is in the second page of the advertisement to the third edition. Now if the Reviewer had given a reasonable attention to the former part of the paragraph in which that observation occurs, he would have found out that I was speaking of the *natural planes*, not the *cleavage planes* of crystals. My words are, "the measurements of the crystalline forms, and especially of the secondary planes, are not precisely exact, do not on all occasions relatively agree.—It has been ascertained by a comparison of the measurements taken from similar and brilliant *planes of different crystals*," [the planes of crystals can be no other than their natural planes] "that, owing to some *natural* inequality of surface, the same precise angle is rarely obtained—that the limit of error is considerably within one degree—that it rarely exceeds 40 minutes, and that it is frequently confined to a minute or two." And if the Reviewer had read a few more lines of the same paragraph, he would have been convinced, even if the above quoted words had not served to convince him, that the exceptions related solely to *natural* planes; for he would have perceived that I have said, "The measurements obtained from *planes produced by cleavage*, may be considered as approximating the truth much more nearly, than those taken by means of the *natural* planes." And if the Reviewer had condescended to peruse page xxix of the Introduction, he would have found this remark; "But if we *cleave a crystal*, carbonate of lime for instance, if it be pure and transparent, we shall find, by the help of the reflective goniometer, that the planes of the primary nucleus which will be extracted, *meet invariably* under angles of 105° 5′ and 74° 55′." The carbonate of lime is here quoted as an instance, that if we *cleave a crystal* [of any substance which is pure] the planes of cleavage will meet under constant angles.

I am now content to leave the reader and the Reviewer himself to judge whether he possesses or not the second and third requisites for his office. I wish, however, that he may be disposed to take these hints for his future government, and believe the fact, that personal feeling is concerned in them only in so far as belongs to the regret necessarily accompanying a misstatement of one's own words. It is for the credit and utility of cleavage and the goniometer principally, that I am induced thus publicly to expose the fallacies of a Reviewer whose dicta on mineralogical systems ought to be received with caution, since he has attributed to an author an error of his own, and which if he had understood the science (he must excuse me) could not have been written.

It might not, perhaps, be very difficult to point out some inaccuracies (to speak gently) in the Reviewer's review of the Systems; but with these I do not meddle. In line 21, p. 491, there is, however, one error that must be noted for the benefit of the Reviewer's reader. For *brimstone* read *limestone*. This error is somewhat odd, considering the locality of its origin, and must be ascribed to the printer, or rather one would think, to the "printer's devil."

Article XII.

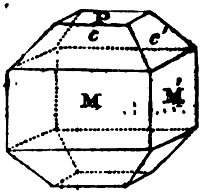
On the Crystalline Forms of Artificial Salts.

By H. J. Brooke, Esq. FRS.

(*Continued from* p. 162.)

Hydrate of Strontia.

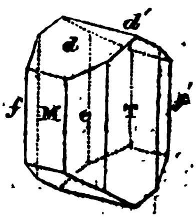
From the measurement and cleavage of some crystals of this substance received from the laboratory of the Royal Institution, I find its primary form to be a right square prism. The cleavage parallel to P is very easy, and the planes bright; that parallel to M and M′ is less determined, although sufficiently apparent.



P on M, or M′	90°	0′
P on c, or c′	137	48
M on M′	90	0
M on c	132	12

Acetate of Strontia.

The crystals I have obtained of this salt from a solution of the carbonate in acetic acid are very small, with rather imperfect planes, and have not afforded *distinct* cleavages parallel to any of these. There is, however, an appearance of cleavage parallel to the plane M of the annexed figure. From the general character of the crystals and from measurement, *a right oblique-angled prism* may be regarded as their primary form. The crystals are very efflorescent.



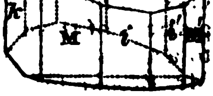
M on T	96°	10′
M on d	107	33
M on f	129	20
M on e	153	12
T on e	122	58
d on d′	124	54

Nitrate of Strontia.—Anhydrous.

The primary form is *a regular octahedron.* The crystals generally resembling those of nitrate of lead, given in the number of the *Annals* for January last.

Hydrous.

A very efflorescent salt, and not affording any distinct cleavage that I have been enabled to discover. We may assume an *oblique rhombic prism* as its primary form. The crystals are sometimes considerably lengthened, and present only those planes which are marked as the primary.



P on M, or M′	103°	40′
P on i, or i′	111	5
P on c	131	47
M on M′	66	20
M on k	146	50
M on i	150	10
i on i′	126	0

ARTICLE XIII.

Analysis of the Nitrates of Strontia, described in the preceding Paper. By Mr. J. T. Cooper.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *March* 20, 1824.

At the request of Mr. Brooke, who observed the difference in the primary forms of the crystals of this salt, I have undertaken its analysis, and for this purpose I exposed 50 grains of the prismatic variety to a heat of 240° Fahr. for a considerable time until it ceased to lose weight; the quantity of water lost was 13·9 grains, leaving 36·1 of dry nitrate; the dry nitrate was then dissolved, and solution of carbonate of potash added (of known strength) until it ceased to precipitate carbonate of strontia; it required a quantity of the solution equal to 23·8 of dry carbonate of potash; the fluid portion now showed no trace of the alkali, or the earth. The carbonate of strontia, when dry, weighed 25·7, and the fluid portion evaporated to dryness left 34·7 of nitrate of potash.

From the above, it is evident the composition of prismatic nitrate of strontia is in 50 grains

Acid	18·4 or in 100	36·8
Base	17·7	35·4
Water	13·9	27·8
	50·0	100·0

This differs considerably from Kirwan who, appears to be the only chemist who has examined this variety: he states the composition of it as,

Acid	31·07
Base	36·21
Water	32·72

The difference in the quantity of water may be in a great measure accounted for by his not selecting perfectly formed crystals for his experiments; but how the variation in the proportions of the acid and base could arise is much more difficult to account for. The crystals employed by me were selected by Mr. Brooke for the purpose.

I also obtained from Mr. Brooke some well-formed crystals of the octahedral variety, and having weighed 14·84 grains, I subjected them as before to heat for a considerable time; the loss amounted to ·07: they were afterwards heated to a much higher temperature, but not sufficient to decompose the salt, when a further loss of ·02 occurred; but this loss was recovered after the salt had stood a few minutes exposed to the air. The crystals did not in the least lose their transparency; they were then dissolved, and solution of carbonate of potash, equivalent to 9·55 of dry carbonate, was added; the fluid portion was then examined for alkali and earth, but showed no traces of either; the carbonate of strontian, when dry, weighed 10·3, and the nitrate of potash 14·23: its composition, therefore, is

Acid................	7·55	or in 100	50·92
Base................	7·22		49·08
	14·77		100·00

The small loss of water, amounting only to ·07, cannot be considered otherwise than interposed water, and not in any way attached to the formation of the crystal: the composition of these salts may be considered as similar, excepting that the octahedral variety contains no water of crystallization, and the prismatic variety 27·8 per cent.

Article XIV.

On a Submarine Forest in the Frith of Tay, with Observations on the Formation of Submarine Forests in General. By John Fleming, DD. FRS. Edin.*

The title which I have given to this paper, is, perhaps, faulty, and apt to lead the imagination to expect a description of the various forms of those sea-weeds which clothe the channel of the deep;—the arrangement of the species, as depending on the soil and depth of water, the food which they yield to the various creatures that browse upon them, and the protection they afford to such as take refuge among their leaves and branches. Very different, however, is the scene which I propose to describe, it being a *bed of peat-moss,* covered by the sea at every full tide, but indicating, by the appearances which it exhibits, that its present low level is different from its original position. In other words, it is a geological phenomenon, occurring in the Frith of Tay, similar to the one observed on the Lincolnshire coast, which, in 1796, was examined by the late Sir Joseph Banks and Dr. Joseph Correa de Serra, and described by the latter in the Transactions of the Royal Society of London for 1799, p. 145, under the title, "On a Submarine Forest, on the East Coast of England." I venture to prefix the same title to this paper, which I now offer to the consideration of the Royal Society of Edinburgh, aware of its impropriety, but urged by the wish to connect similar phenomena by the common terms employed in their description.

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* From the Transactions of the Royal Society of Edinburgh, vol. ix. Part II.

The bed of peat to be described, and now dignified by the title of a *Submarine Forest*, occurs on the south bank of the Frith of Tay, and has been observed in detached portions on the west side of Flisk Beach, to the extent of nearly three miles, and on the east side, upwards of seven miles. At this particular place, to which the following observations chiefly apply, it rests upon a bed of clay of unknown depth. This clay is of a grey colour, much mixed with mica, and in some places with grains of quartz, and resembles the carse ground on the opposite side of the Frith, or the contents of the sand-banks which obstruct its channel. The upper portion of this clay has been penetrated by numerous roots, which are now changed into peat, and some of them even into iron pyrites. The surface of this bed is horizontal, and situate nearly on a level with low water-mark. In this respect, however, it varies a little in different places. The peat-bed occurs immediately above this clay. It consists of the remains of the leaves, stems, and roots, of various common plants, of the natural orders Equisetaceæ, Gramineæ, and Cyperaceæ, mixed with roots, leaves, and branches of birch, hazel, and probably also alder. Hazel nuts, destitute of kernel, are of frequent occurrence. All these vegetable remains are much depressed or flattened, where they occur in a horizontal position, but, where vertical, they retain their original rounded form. The peat can be easily separated into thin layers, the surface of each covered with leaves. The lowest portion of this peat is of a browner colour than the superior layers; the texture likewise is more compact, and the vegetable remains more obliterated. The peat contains a good deal of earthy matter.

The surface of this bed of peat is nowhere (that I have detected) covered by any alluvial stratum, nor does it occur at a higher level than four or five feet below high water-mark. Towards the shore it seems to be cut off by the old red alluvial clay, on which the newer grey, or carse clay also rests.

The only circumstance of much interest, in reference to this peat-bed, remains to be stated. Upon its surface may be perceived the stumps of trees, with the roots attached, and evidently occupying the position in which they formerly grew; as the roots are observed to spread, subdivide, and penetrate the bed in their usual natural manner. I have counted at one time, after a favourable tide had cleared away all silt and gravel from the surface, upwards of a score of these roots, situate at unequal distances from one another, but all, by the position and arrangement of their roots, demonstrating that such had been, while growing, their original situation. To prevent any suspicion from arising, that I may have been deceived on this subject, I may state, that the scene, situate but a few hundred yards from my dwelling, has been examined repeatedly, and under different circumstances, and several friends who have visited the spot, have appeared satisfied of the accuracy of my conclusions. I may mention the names of two of these, Mr. Neill and Mr. Bald, both members of this Society, and well qualified, by habits of observation, to form a correct opinion on the subject. Many of these trunks and roots occur from eight to ten feet below high water-mark.

If we assume, therefore, that the roots of these trees are in their natural position, with respect to the bed which now supports them, are we warranted to conclude that they grew on a surface ten feet lower than the high water-mark, but before that surface was exposed to the periodical inundations of the tide? Every cavity, in this climate, situate at a lower level than that of the sea, is invariably filled with water, and in a condition hostile to the growth of trees, until its surface has been elevated, by the washing in of mud, or the growth of peat, to a position at least equal to the ordinary rise of the tide. Since these trees could not, therefore, have grown in an inland valley so far below the rise of the tide, even where the sea was excluded, we must draw the conclusion that the surface on which these trees grew, was, at the period of their growth, at least ten feet higher, in relation to the sea, than at present; and to account for this remarkable change, we must adopt one of the following suppositions:—Either that the sea has risen ten feet, and overflowed that surface which was formerly beyond its reach; or, that the ground supporting these trees has sunk to the same extent.

The first of these suppositions, viz. a permanent rising of the sea, has not been resorted to by any of those writers whom we have had an opportunity of consulting. Indeed it is contrary to those known laws which regulate the movements of the ocean, and receives no support from any circumstances which have been observed on the maritime shores of this country.

If, then, we abandon the idea that the sea has gained an elevation of its level, and adopt the other supposition, viz. that the peat-bed has sunk, so as now to be ten feet lower than when the trees grew upon its surface, we advance a step nearer the object at which we aim. It still remains, however, to be determined, what those causes were, which operated in depressing the surface of this bed, and enabling the waves to pass over that soil which was formerly so much beyond their influence, as to be fit for the support of the hazel and the birch tree.

The first method of explaining the phenomenon likely to present itself, especially where the bed is limited in extent, is by supposing that the substratum, having lost its adhesion to the bed on which it rested, by the percolation of water, and the exposure of the side next the sea, moved down an inclined plane into deep water, carrying along with it the upper layer of vegetable matter, and the trees growing upon its surface. Such occurrences have taken place in several inland bogs, both in Scotland and Ireland, which have moved out of their positions to a lower level. The extent, however, of this bed, and the horizontality of its layers, prevent us from considering its present depressed position as having been produced by any sliding of this kind. Neither hath it arisen from the washing away of the soft matter on which the bed supporting the trees rested, for the clay still remains, and at the line of junction is much incorporated with the peat.

This washing away of the subsoil, however, has been resorted to by Mr. Watt of Skail, to explain the conditions of a submarine forest on the west coast of Orkney. It occurred to him "that this bed of moss and trees has arrived at its present level (so as to be covered, during the flood-tide, to the depth of at least fifteen feet of water), in consequence of the removal of a bank of earth, at least eighteen feet deep, which has been washed gradually away, by the water of the Loch of Skaill oozing along the rocks upon which it rested, and upon which the mass of leaves now rests, held together by the fibres of the roots of the trees." See Edinb. Phil. Jour., vol. iii. p. 101. This explanation, however, is liable to very strong objections. It is not probable, that, on the stormy coast of the west side of Orkney, where the rocks themselves yield to the fury of the waves, and where the top of every cliff is a heap of ruins, a mass of earth, eighteen feet in thickness, would be permitted to remain, until washed away by the slow force of percolating fresh water, or that a continuous bed of peat, of nearly an acre in extent, would be spared from destruction, and suffered to settle peacefully, in the Bay of Skaill, so as to be covered at flood-tide with fifteen feet of water.

If we have no reason to believe that this Tay-bed was transported to its present situation, in what manner has it reached its present level? Two solutions of this curious question have been offered, as connected with similar occurrences, by eminent individuals, Dr. Borlase, Dr. J. Correa de Serra, and Professor Playfair.

Dr. Borlase, who, in 1757, observed a submarine forest at Mount's Bay, Cornwall, covered at full tide with twelve feet of water, considered the depression of the bed, which supported the trees, and still contained their roots *in situ*, as having arisen from subsidence of the ground, produced by earthquakes, or, to state it in his own words, "that there has been a subsidence of the sea-shore hereabouts, is hinted in my letter to you, p. 92; and the different levels and tendencies which we observed in the positions of the trees we found, afford us some material inferences as to the degree and inequalities of such subsidences in general; as the age in which this subsidence happened (near 1000 years since at least), may convince us, that when earthquakes happen, it is well for the country that they are attended with subsidences; for then the ground settles, and the inflammable matter, which occasioned the earthquake, has no longer room to spread, unite and recruit its forces, so as to create frequent and subsequent earthquakes; whereas, where there are earthquakes without proportional subsidences, there the caverns and ducts under ground remaining open and unchoaked, the same cause which occasioned the first has room to revive, and renew its struggles, and to repeat its desolations and terrors; which is most probably the case of Lisbon." Phil. Trans. 1757, p. 52.

The views of Dr. Borlase, in reference to this depression of the ground, in consequence of earthquakes, was evidently influenced by the curious observations which he had formerly made on the subsidence of some places at the Scilly Islands, as stated, Phil. Trans. vol. xlviii. p. 62; and other observers may be led to form the same opinion, especially if the singular sinking of the cliff at Folkstone, about forty feet, even in the absence of an earthquake, be taken in consideration. See Phil. Trans. 1786, p. 220.

Dr. Correa de Serra also ascribes the depressed position of the submarine forest of Lincolnshire to the force of subsidence, aided by the sudden action of earthquakes. "There is a force of subsidence (he says) (particularly in soft ground), which, being a natural consequence of gravity, slowly, though perpetually operating, has its action sometimes quickened and rendered sudden by extraneous causes, for instance, by earthquakes." "This force of subsidence, suddenly acting by means of some earthquake, seems to me the most probable cause to which the actual submarine situation of the forest we are speaking of may be ascribed. It affords a simple easy explanation of the matter; its probability is supported by numberless instances of similar events." Phil. Trans. 1799.

Professor Playfair, when contemplating the phenomena of the Lincolnshire submarine forest, rejected the explanation offered by Dr. Correa de Serra, and availed himself of some of the peculiar assumptions of the Huttonian Theory of the Earth. "The subsidence (he says) however, is not here understood to arise from the mere yielding of some of the strata immediately underneath, but is conceived to be a part of that geological system of alternate depression and elevation of the surface, which probably extends to the whole mineral kingdom. To reconcile all the different facts, I should be tempted to think, that the forest which once covered Lincolnshire, was immersed under the sea by the subsidence of the land to a great depth, and at a period considerably remote; that when so immersed, it was covered over with the bed of clay which now lies upon it, by deposition from the sea, and the washing down of earth from the land; that it has emerged from this great depth till a part of it has become dry land; but that it is now sinking again, if the tradition of the country deserves any credit; that the part of it in the sea is deeper under water at present than it was a few years ago." Illustrations of the Huttonian Theory, p. 453.

A careful examination of these conjectures, which had been offered to account for the phenomena of submarine forests, soon convinced me that the subject was still imperfectly understood. Under this impression, I endeavoured to become possessed of all the conditions of the problem, and now venture to offer a solution. The opinion which I have been led to form has been entertained for some years, and stated to several friends, without an objection having presented itself.

If we suppose a lake situate near the sea-shore, and having its outlet elevated a few feet above the rise of the tide, we have the first condition requisite for the production of a submarine forest.

If we now suppose, that, by means of mud carried in by rivulets, and the growth of aquatic plants, this lake has become a marsh, and a stratum of vegetable matter formed on the surface, of sufficient density to support trees, we arrive at the second condition which is requisite. This state of a marsh, formerly a lake, is of common occurrence, more especially where the surrounding grounds are high, and covered with soil, for in this case the rain washes down earthy particles, and, by spreading them on the grassy surface, renders it a more suitable soil for the growth of trees.

In this second condition, all the strata below the outlet of the marsh are kept constantly wet, or in a semifluid state. The force of ordinary subsidence, aided by occasional earthquakes, may render the whole tolerably compact; yet the quantity of water necessarily present, will prevent any thing like the degree of condensation of ordinary alluvial land or soil from taking place.

Suppose a marsh in this condition to have the level of its outlet lowered, or rather, to have its seaward barrier removed (an occurrence which many circumstances induce us to believe to have happened frequently both on the east and west coasts of this country, where submarine forests are not of rare occurrence), what consequences would follow? The extremities of the strata now exposed to the sea, would at every ebb-tide be left dry, to a depth equal to the fall of the tide. Much water, formerly prevented from escaping by the altitude of the outlet, would now ooze out from the moist beds, and the subsiding force would act more powerfully in the absence of the water which filled every pore. All the strata above low water-mark would thus collapse, and the surface of the marsh, instead of remaining at its original height, would sink below the level of the sea. But the escape of the water from the strata would not, in such circumstances, be confined to the beds situate above the low water-line. Even those occupying a position considerably lower, would be influenced by the change; for the water even in such would be squeezed out, in consequence of the pressure of all the matter of the strata above the low water-mark, exerted during every ebb, in the expulsion of the water at the lowest level, thus permitting the subsidence of the strata to take place even to the lowest beds of the morass.

In consequence of this drainage, produced by the ebbing of the tide on those marshes, the original barriers of which have been destroyed, there is no difficulty in accounting for the depression of the surface of a marsh many feet lower than its original level, nor in explaining the fact that Neptune now triumphs where Sylvanus reigned, and that the sprightly Nereids now occupy the dwellings of their sister Naids.

The same explanation, now offered to account for the submarine forest of the Tay, seems equally applicable to those of Mount's Bay, Lincolnshire, and Orkney. It is warranted by the effects which we have observed to have taken place in different districts of Scotland, from the artificial drainage of marshes which had formerly been lakes, and which were in a condition of surface fit for the growth both of willows and alders. In some cases, where the outlet of the marsh has been lowered perhaps ten feet, and a ditch at this new level opened through the middle of the ground, an expectation has been formed that the original surface would be drained of all its moisture, and brought into an arable condition. A season, however, has scarcely elapsed, before this deep ditch has become shallow, not by the silting up of the bottom, but by the subsidence of the neighbouring matter, in consequence of the abstraction of the water; and the ground which was expected to become fit for yielding crops of grain, has returned to a condition better suited to the growth of rushes. No provision in these cases had been made for the effect of subsidence.

Before concluding this paper, I may take notice of a few facts which seem to have some interest in a geological point of view.

1. One effect of the subsidence to which I have here alluded, is the complanation of all the vegetable remains which occur in a horizontal position, or parallel with the surface of the bed of peat; while those situate vertically retain their cylindrical shape. The vegetable remains, so common in the strata accompanying coal in this country, exhibit the same appearances in similar circumstances, and lead to the conclusion, that the matter of the strata, at the period of deposition, was in such a

condition as to admit of the mechanical effects of subsidence taking place.

2. In the examination of the vegetable remains in this bed of peat, and of others which have been investigated, I have been led to conclude (contrary to the commonly received opinion [*]), that many of the supposed stems of reeds which occur in a petrified state, are in fact roots. These roots, or rather subterranean stems, such as the Arundo colorata and phragmites, Menyanthes trifoliata, and many other marsh plants exhibit, frequently occur in beds of peat, in a dead state, and exhibit their peculiar characters, when but few traces of the stems to which they belonged can be detected.

3. Several changes of a chemical kind have already taken place in this stratum of peat. The fibrous structure of much of the vegetable matter is obliterated, small portions of the reeds, and even of the wood, are so changed as to resemble wood-coal;—changes these, which plainly intimate, that a process is going on, by which, in time, that which is now peat may become coal. In the crevices of some portions of the wood I have detected thin crusts of the blue phosphate of iron. It is rather singular to have found some of the roots in the soft clay changed into iron-pyrites. This change has chiefly taken place in the bark, and in such cases the wood and pith are wanting. In one example, however, the pith remained, and had likewise been converted into pyrites. In many cases the clay is full of tubular cavities, the remains of the spaces which the roots or stems of plants once occupied. The walls of these cavities are usually of a darker colour, and firmer texture, than the surrounding matter, and have evidently undergone some change, in consequence of the decomposition of the vegetable matter. In some cases, the epidermis of the plant remains, in contact with the surrounding clay, while the matter of the interior has disappeared. Into these cavities the clayey matter enters slowly, and fills the mould which the decomposition of the plant has prepared. This may be regarded as a process similar to the one which has taken place in those vegetable petrifactions so common in the argillaceous and arenaceous beds of the coal-formation, in which slate-clay, clay-ironstone and sandstone, are exhibited under the external forms of plants.

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[*] See Parkinson's Organic Remains, vol. i. p. 455.

ARTICLE XV.

ANALYSES OF BOOKS.

Philosophical Transactions of the Royal Society of London, for 1823. Part II.

(*Concluded from p.* [illegible].)

XXVI. *On the Apparent Magnetism of Metallic Titanium.* By William Hyde Wollaston, MD. VPRS.

This brief but interesting communication is as follows:—

"In an account that I lately gave of the properties of metallic titanium, which is printed in the first part of the volume of the Philosophical Transactions for the present year,* there is an oversight, which I am desirous of rectifying as soon as may be. I have there stated that the cubic crystals of titanium, when first detached from the iron-slag where they are found, were all attracted by a magnet, but that when they had been freed from all particles of iron adherent to them, they appeared to be no longer acted upon by it.

"Having since that time been led by the observations of M. Peschier, of Geneva, to examine this question more accurately, I find that, although the crystals are not sufficiently attractile to be wholly supported by the magnet, yet when a crystal is supported by a fine thread, the force of attraction is sufficient to draw it about 20° from the perpendicular, and consequently that the force of attraction is equal to about one-third the weight of the metal.

"When a piece of soft iron of about the same size was made of a cubic form (weighing half a grain), the attractive force of the iron to the same magnet was found, in successive trials, to lift from eighty to ninety times its weight of a silver chain adapted to this inquiry.

"By a similar mode of trial, I found that cobalt carried from fifty to sixty times its weight, and that a similar quantity of nickel supported from twenty to thirty times its own weight by the same magnet.

"From the above comparison of the magnetic forces, it is evident that the presence of about 1-250th part of iron as an alloy in the metallic titanium, would be sufficient to account for this power, without regarding titanium itself as a magnetic metal; and its origin in the midst of iron, gives every reason to suspect that it would be contaminated by some proportion of that metal.

"It is, however, extremely difficult really to detect the presence of so small a proportion of iron, on account of the high colour of the precipitates of titanium. For though it may be easy to produce an appearance of blue by using a prussiate, which already contains iron, and is consequently better adapted to prove the absence of iron where no blueness appears, than to ascertain its presence, it is by no means easy to obtain the more indisputable evidence of iron by infusion of galls. It is only by repeated evaporation of the muriatic solution, and continued exposure of the residuum to the temperature of boiling water, that I have succeeded in separating enough of the titanium to allow the blackness of gallate of iron to appear, when the efflorescent edges of the dried salt are touched with infusion of galls.

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* Annals, N. S. v. 67; vi. 222.

"Although the quantity thus rendered sensible does not appear in proportion sufficient to account for the magnetic force observed, there seems more reason to ascribe it to this impurity, than to suppose titanium possessed of that peculiar property in a degree so far inferior to the other known magnetic metals."

XXVII. *An Account of the Effect of Mercurial Vapours on the Crew of his Majesty's Ship Triumph, in the Year* 1810. By William Burnett, MD. one of the Medical Commissioners of the Navy, formerly Physician and Inspector of Hospitals to the Mediterranean Fleet. Communicated by Matthew Baillie, MD. FRS.

The curious facts to which this paper relates, having already been detailed by Dr. Paris and Mr. Fonblanque, in their "Medical Jurisprudence," vol. ii. p. 460, and the paper itself having been reprinted in a contemporary journal, their repetition here is unnecessary.

XXVIII. *On the Astronomical Refractions.* By J. Ivory, AM. FRS.

We have endeavoured, in the following paragraphs, to convey a general idea of the nature and contents of this voluminous and elaborate communication, necessarily omitting the author's mathematical details, but adopting, for the most part, his own words.

It was known to the ancient astronomers that there is a difference between the real and apparent places of the stars, arising from the refraction of light in its passage through the atmosphere. Tycho Brahé was the first who attempted to free his observations from the effect of this irregularity. Since his time the astronomical refraction has become more and more an object of attention, as it is found to have the greatest influence on the delicate exactness of modern observations. In the course of the last twenty years many researches on this subject have been published by philosophers of the first note, who have applied all the resources both of theory and practice to overcome the difficulties which it presents. By these means our knowledge has been greatly extended; but the problem of the refractions must still be considered as the most imperfect part of modern astronomy.

The first investigation that presents itself in the problem of the refractions is to find the velocity of the light at any given height in the atmosphere. The only physical principle wanted for this purpose is the refractive power of air according to its density. Hauksbee first determined by experiment, that air refracts light in proportion to its density; and this result has been confirmed by succeeding philosophers. There is even good reason to think, that the conclusion of Hauksbee is not materially affected by the variable quantities of aqueous vapour contained in the atmosphere at different times. Admitting then the principle we have mentioned, we must conceive that the light coming from the sun or from a star, moves in vacuo with a uniform velocity until it reaches the atmosphere. It is there deflected from its course by the spherical and concentric shells of air it meets with, each of which acts upon it with a force perpendicular to its surface, and directed to the centre of the earth: Now, as all the light enters the atmosphere with the same velocity, and as the deflecting forces are of the same intensity at the same distance from the common centre to which they tend; it follows, that the new velocities acquired by the action of the forces will be independent of the direction of the light's motion, and will be the same at the same distance above the earth's surface.

The ultimate deviation of the light of a star from its primitive direction depends upon the augmentation of the velocity which the light acquires in its passage through the atmosphere, and likewise upon the different obliquities with which it crosses the several strata of air. Now the first of these two things is the same for all stars and for all constitutions of the atmosphere, for it is the same when the density of the lowest stratum of air continues the same. But the second is different for stars, that are differently placed with regard to the zenith, and it varies also with the densities of the strata that compose the atmosphere. It is, therefore, certain, that the formula of Laplace is rigorously exact in no case whatever. But when a star is near the zenith, the variations in the obliquity of the light in passing through the several strata of air, are inconsiderable; and the formula will be nearly true. However, there is always some error, which accumulates as the zenith distance increases, and will at length become sensible. Delambre tells us, that in comparing the observations of different days, he found errors arising from refraction that amounted to *6″* or *7″* at 75° from the zenith; and the observations of a very accurate astronomer show that similar inequalities are perceptible much nearer the zenith (Dr. Brinkley's paper Phil. Trans. 1821, p. 342). Now these inequalities do not arise from any thing imperfect in the manner of observing: they are undoubtedly produced by alterations in the remote parts of the atmosphere which do not affect the barometer or thermometer placed at the observatory. It appears, therefore, that the peculiar constitution of the atmosphere has a perceptible influence on the refraction at 75° from the zenith; and when

Laplace's formula is made to extend to 74°, it is carried to its utmost limit. However mutable we may suppose the condition of the atmosphere to be, there must be a mean state equally removed from the opposite extremes. Now, a table of refractions that should have this mean state of the atmosphere for its basis would be the most advantageous of any. For although with respect to single observations, the errors of such a table might be as great as in some other hypotheses, yet in a numerous set of observations made at different times so as to embrace all the usual changes, the inequalities of an opposite kind would counterbalance one another. But to a certain distance from the zenith, Laplace's formula is sufficiently exact for practical purposes; and it has the advantage of taking away the necessity of having recourse to precarious suppositions respecting the constitution of the atmosphere.

There is no ground in experience for attributing to the gradation of heat in the atmosphere any other law than that of an equable decrease as the altitude increases. This law prevails to the greatest heights to which we have been able to ascend. The mean elevation for one degree of depression of the centigrade thermometer is very nearly 90 English fathoms; and in the height ascended by Gay-Lussac, rather more than 4¼ miles, the same quantity comes out equal to 95 fathoms. To this great extent the law of a uniform decrease of temperature holds good without much deviation from the truth. It therefore seems to be the assumption most likely to guide us aright in approximating to the true constitution of the atmosphere. The very exact coincidence in the properties of all the atmospheres comprehended in the formula assumed by Mr. Ivory, with the phenomena actually observed at the surface of the earth, accounts in a satisfactory manner for the near approach of the refractions in every case to the quantities determined by astronomers. It appears that although the refractions near the zenith are affected in a degree hardly perceptible by the peculiar constitution of the atmosphere, yet near the horizon they depend upon the same arrangement of the strata of air indicated by terrestrial experiments. The causes in the irregularities observable in these last, likewise disturb the celestial phænomenon in a more remarkable manner. In measuring the height of a column of air, the accidental disturbances to which the atmosphere is continually subject, are in some measure corrected by means of the temperatures observed at both extremities of the column; but in computing the refractions, the astronomer has no guide but the thermometer placed in his observatory. In the remote parts of the atmosphere, there may occur innumerable changes deflecting the light of a star from its proper course, of which he has no intimation, and for which he can make no allowance. In comparison of this great source of error, we may reckon as of small account the inaccuracies that are owing to the neglect of moisture diffused in the atmosphere, or to our want of an exact knowledge of the law of density in regard to temperature. There can hardly be any other remedy than that of which astronomers so often avail themselves, whenever an ignorance of the real causes obliges them to assimilate the phænomena to the effect of chance; namely, to multiply observations in different circumstances, with the view of making the inequalities of an opposite description compensate one another.

From the foregoing discussion we may draw this conclusion: that an atmosphere constituted like that of the earth must have an altitude of at least 25 miles in order that the refractions from the zenith to the horizon be such as they are actually observed to be. But an atmosphere agreeing with nature in the quantity of the refractions may be found, that shall have any proposed altitude greater than the minimum quantity; and we may infer from the duration of twilight, that the atmosphere of the earth must have an altitude equal to 50 miles, or even more.

XXIX. *Observations on Air found in the Pleura, in a Case of Pneumato-thorax; with Experiments on the Absorption of different Kinds of Air introduced into the Pleura.* By John Davy, MD, FRS.

A brief abstract of this paper will be found in our report of the proceedings of the Royal Society, in the *Annals* for July, 1823. Dr. Davy has added, in an Appendix, an account of a case of pneumato-thorax, in which the operation of tapping the chest was performed; with some additional observations on air found within the body; and on the power of mucous membranes to absorb air. The air collected in this case, by tapping the chest, consisted of 93 azotic gas, and 7 carbonic acid gas; thus, in composition, proving almost exactly the same as the air found in the fatal case described in the Paper itself. Various facts are adduced, tending to support the idea, that mucous membranes are capable of absorbing air.

XXX. *On Bitumen in Stones.* By the Right Hon. George Knox, FRS.

The results of the experiments detailed by Mr. Knox in this paper, possess considerable interest, particularly as indicating the propriety of instituting further researches on the subject; and of examining stony substances for bitumen when they are submitted to analysis: we therefore subjoin them, in a tabular form.

Minerals.	Their localities.	Loss by ignit. p. cent.	Product by distillation per cent.	Residuum of distillation.—Remarks.
1. Pitchstone.	Arran	4·7059	4·5 water and bitumen	Residuum a substance resembling pumice, breaking into convex and concave joints, like those in basaltic columns; colour changed from dark oil-green to milk-white.
2. Pearlstone.	Tokay	3·25	2·5 ditto	Ditto a soft pumice, breaking as above; colour changed from ash-grey to reddish-white.
3. Pumice.	Iceland			Fused without loss.
4. Amygdaloid.	Disco Island	3·25	3·1 bituminous water	
5. Basaltic greenstone.	Newry	6·25	1·75 pure bitumen	The pure bitumen was obtained after the expulsion of the water by a heat below redness.
6. Transition greenstone.	Carlingford, Louth	2	1·5 chiefly bitumen	
7. Bole.	Disco Island	24·5	Bitumen	Quantity of bitumen not ascertained; it had a saline taste, reddened turmeric paper, and gave dense fumes with vapour of muriatic acid. Colour changed from Isabella-yellow to tile-red.
8. Basalt.	Giant's Causeway	6·051	6 bitumen and water	Residuum a pumice, breaking as in No. 1.
9. Ditto	Disco Island		2·312 ditto	Ditto.
10. Obsidian	Lipari	1·75	0·2 bituminous water	Residuum a substance resembling the glassy pumice, which occurs with the obsidian of Ascension Island, and breaking as No. 1: bituminous water gave indications of ammonia.
11. Greenstone	Clack Hill	2	2 chiefly bitumen	Part of the bitumen very volatile.
12. Pure wacke.	Disco Island	19·4	11·42 bituminous water	Besides the bituminous water distilled over, there were 4 cubic inches carbonic acid, and 8 carburetted hydrogen. Residuum a velvet-black powder unchanged by a heat that fused cast iron; in an open vessel, lost 4 per cent. of carbon, and then melted into slag.*
13. Iron clay.	Ditto	21	18·25 ditto	
14. Ditto	Howth	5	4 chiefly bitumen	Residuum a pale violet-blue pumice, approaching to vitrification.

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* Mr. Knox was induced by the above experiments to try whether, by igniting this substance under charcoal to prevent the escape of the carbon, he might not make black chalk. The experiment was successful. The chalk was of a pretty good consistence and colour, and was effaced like graphite, both by bread and caoutchouc.

Minerals.	Their localities.	Loss by ignit. p. cent.	Product by distillation per cent.	Residuum of distillation.—Remarks.
15. Bole	Disco Island			Result of distillation nearly as in No. 7, but it contained no ammonia.
16. Hornblende	Schneeburg, Up. Sax.	3	0·75 bituminous water	Some of the bituminous water lost in the process.
17. Tourmaline	Karorulik*		0·7 ditto	Pumice.
18. Augite	Arendal	0·5	0·35 chiefly bitumen	Residuum a powder; heat not very great.
19. Common serpentine	Zeplitz, Upp. Saxony	18·5	10·5 bituminous water	
20. Clayslate	Bangor	3·25	3 ditto	Ditto coherent; not hard; broke as in No. 1.
21. 22. White felspar	Killiney	0·40	0·35 ditto	Water gave indications of ammonia. Same results, but no appearance of ammonia.
23. Flesh-red ditto	Aberdeen		Bituminous water	Residuum pale reddish-white, coherent, but friable.
24. Menilite	Menil Montant		3·75 ditto	Residuum friable; lost 1·25 by stronger heat.†
25. Adhesive slate	Ditto		18·5 water and bitumen	Residuum coherent, but friable; broke as No. 1.
26. Mica slate	Freyburg, Saxony	2	Bituminous water	Bituminous water very volatile; no ammonia. Residuum a heavy pumice; broke as No. 1.
27. Mica	The Urals, Siberia		1·33 ditto	With indications of ammonia; residuum slightly coherent.
28. Fetid quartz	Nantes, France	0·937	Bituminous water	Product smelled strongly of naphtha, but contained neither ammonia nor sulph. hyd. though the stone lost its smell by ignition.
29. Common ditto		1	0·1 very fetid ditto	Residuum an unaltered powder.
30. Rock crystal	Cape of Good Hope	0		
31. Adularia ‡		0		
32. Ditto §	Greenland	0·4		
33. Marble	Carrara		0·15 mere water	
34. Lucullite	Galway		0·188	Oily smell at first, which afterwards became ammoniacal; reddened litmus paper became blue, and vegetable blue paper green.‖

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[*] In West Greenland. [†] Ignited under charcoal it became black, but did not write. [‡] Pearl-white. [§] Pearl-blue.

[‖] In a distillation of lucullite in which the lime was not rendered quite caustic, muriatic acid in dissolving the residuum produced a smell of sulphuretted hydrogen.

XXXI. *On certain Changes which appear to have taken place in the Position of some of the principal Fixed Stars.* By John Pond, Esq. Astronomer Royal, FRS.

This communication chiefly consists of a series of tables of observations on the stars; confirming, in Mr. Pond's judgment, his doctrine of their southern motion.—B.

Article XVI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Feb. 19.—A Series of Meteorological and Astronomical Observations made in New South Wales, and on the Voyage to that Country, were presented from Major-Gen. Sir T. Brisbane, KCB. FRS.

A paper was read, "On the Semi-decussation of the Optic Nerves; by W. H. Wollaston, MD. VPRS."

It has been generally concluded by anatomists, and they support the conclusion from the observation of the arrangement of the optic nerves as distinctly seen in certain kinds of fishes, that in the human eye, the optic nerves, after passing from the thalami nervorum opticorum, meet, and then proceed apparently in union, though in reality still separate; so that the right eye is believed to be entirely supplied with these nerves from the left thalamus, and the left eye from the right thalamus; and this arrangement is called the *decussation of the optic nerves*. The consideration of a particular species of blindness, however, has led Dr. Wollaston to a somewhat different distribution of the optic nerves. After fatigue, arising from four or five hours' violent exercise, Dr. Wollaston was affected by a partial blindness, of which he first became sensible by seeing only half the face of a person near him, and next by seeing only the termination "son" of the name "Johnson;" this blindness was to the left of the point of vision in each eye; it was not perfect darkness, but merely a dark shade; and in about fifteen minutes, it gradually passed off, in an oblique direction upwards towards the left. As it was referable to an affection of the nerves, Dr. W. did not apprehend or experience any return of it, other nervous affections being produced by fatigue. Some years afterwards he again experienced this singular kind of blindness, without any obvious cause, and first became sensible of it likewise by seeing only the half of a person's face; but in this case the right side of both eyes was affected, and complete vision was suddenly restored by the joy produced on receiving information of the safe arrival of a friend from a hazardous enterprise. Dr. Wollaston has a friend who has experienced the same affection for seventeen years past, whenever his stomach is considerably deranged: another friend was attacked by pain at the left temple, and at the back of the left eye, which was succeeded by this sort of blindness on the right side of each eye; he can see to write,—see the paper he is writing upon, and the pen he writes with,—but not the hand that guides the pen. The affection in this case, Dr. W. fears, is a permanent one; the pain first experienced seems to have arisen from some effusion causing a degree of pressure on the brain, and the blindness from the continuance of this pressure on the left thalamus nervorum opticorum.

Now all these cases seem referable to the partial insensibility of each retina, and they indicate that the left side of the retina in each eye is supplied with nerves from the same thalamus, and the right from the opposite thalamus; so that the nerves supplying the former alone decussate, and not those of the right side; an arrangement which Dr. Wollaston calls the *semi-decussation of the optic nerves*.

Dr. Wollaston proceeded to illustrate this statement of the distribution of the optic nerves, from that observed in those of fishes: in the sturgeon the eyes are diametrically opposite each other, each on one side of the head, the left eye being entirely supplied with nerves from the left thalamus of the brain, and the right eye entirely from the right thalamus. The blindness above described, Dr. W. remarked, does not appear to be rare, but is seldom particularly noticed, like many other things, because it is not understood.

This very interesting paper concluded with a short section in which Dr. Wollaston applies the sympathy of structure in the eyes, indicated by the effects just noticed, to the explanation of the long agitated question respecting the cause of single vision with two eyes. Every point in each eye is supplied with a pair of filaments from the same nerve, and the two eyes thus sympathize with each other in every point: hence arises single vision; and hence also the reason why infants direct both eyes in a corresponding direction, instead of squinting.

Feb. 26.—A Catalogue was presented from the Rev. Fearon Fallows, FRS. Astronomer at the Cape of Good Hope, of nearly all the principal Fixed Stars between the zenith of Cape Town, Cape of Good Hope, and the South Pole.

A paper was read, "On the various Degrees of Intensity of the local Magnetic Attraction in different Parts of Ships; by George Harvey, MAS. Communicated by John Barrow, Esq. FRS. Secretary to the Admiralty."

March 4.—William Wavell, MD. and Capt. Philip Parker King, RN. were admitted Fellows of the Society; the Lord Bishop of Limerick, and the Rev. Dr. E. Maltby, Prebendary of Lincoln, being unable to attend for admission, requested to have their names inserted in the printed lists of the Society; which was granted accordingly.

A letter to the President was read, from Sir E. Home, Bart. VPRS. entitled "Some curious Facts respecting the Walrus and Seal, discovered in the Examination of Specimens brought Home by the late Expeditions, from the Polar Circle."

As the late various expeditions to the northern regions had been planned, primarily, by the President and Council of the Royal Society, Sir Everard Home wished to lay before the Society some curious facts which he had ascertained in the examination of some specimens brought home by them. This he was desirous of doing before the officers who are to proceed on the new expeditions should have left our coasts, in order that they might know that their exertions were important to science in various respects, besides the grand objects of their researches; and that they might likewise know that the pickle or brine in which provisions are preserved at sea is well adapted to the preservation of the internal parts of animals, preserving them in a better state for examination, dissection, and injection, than when they have been long steeped in spirits.

The first discovery Sir Everard had to state was, that the hind-flipper or foot of the walrus is provided with means for enabling the animal to walk in opposition to gravity precisely analogous to those possessed by the fly, and the use of which could not have been suspected, had not the previous discovery been made respecting the latter animal, as described in the Phil. Trans. for 1816. Sir Everard at once recognized this structure on seeing a mutilated foot of the walrus, and, in consequence, had requested his friend Capt. Sabine to procure him a specimen of the animal, which Capt. S. had accordingly done, with the aid of the assistant-surgeon of the vessel in which he sailed. The examination of this specimen showed, that in the hind foot of the walrus there is a cup for enabling the animal to produce a vacuum, and thus to walk in opposition to gravity exactly like the two cups with which the fly's foot is provided. The apparatus in the latter required magnifying 100 times to make the cups distinctly visible, but in the walrus it was diminished four times to bring it within the compass of a quarto plate. The author, when writing his former papers on the fly's means of progression, had not been able to determine the use of the two points in the foot of that animal; Mr. Adams had called them *pickers*, and had supposed that they were inserted in the cavities of the surface over which the animal was walking, and thus retained it in opposition to gravity,—an opinion which Sir Everard Home deemed undeserving of consideration; though he could not assign any use to the points in question. In the foot of the walrus, however, it is evident that the two toes which answer to the points in that of the fly are used for the purpose of bringing the web closely down upon the surface traversed, so as to enable the animal to form a more perfect vacuum; and that the air is re-admitted on their being lifted up.

This part of the paper was illustrated by a drawing by Mr. Bauer; and it was singular, Sir Everard observed, that that gentleman should have had to delineate the same organ in two such different animals.

The second fact described in this paper also relates to the walrus. The bile in this animal is received from the liver by a lateral communication into a cylindrical reservoir, with much mucus in its coats, and is thence impelled with considerable force into the duodenum. The œsophagus is wide, admitting of large masses of food being swallowed, and of regurgitation: the opening of the pylorus is small and valvular, preventing the passage of its contents back again into the duodenum: the structure of the duodenum, pylorus, and adjacent organs, is very similar to that of those of the seal. It had been observed by Mr. Fisher, the astronomer to the late expedition under Capt. Parry, that the food of the walrus is the *fucus digitatus*, which is found in great abundance in the Arctic seas, thrown up on the shores by the waves, and also beneath the ice.

The third fact to which Sir Everard Home adverted in this communication relates to the structure of the funis and placenta of the seal, as observed in a specimen of those parts brought home by Lieut. Griffiths, one of the officers in the late expedition under Capt. Parry. The vessels composing the former are not twisted, and are about nine inches long; at the distance of three inches from the placenta, they anastomoze into blood-vessels, which are connected with the placenta by three membranous coats; the whole conformation giving great freedom to the embryonic circulation. Drawings of this subject and that last noticed, made by Mr. Rose, a pupil under the author at St. George's Hospital, were annexed to the paper.

A communication was likewise read, entitled "Some further Particulars of a Case of Pneumato-thorax; by J. Davy, MD. FRS." Of this we shall give an account in our next.

ASTRONOMICAL SOCIETY.

Feb. 13.—This day being the fourth anniversary of the Society, a numerous meeting of the members took place, at the apartments in Lincoln's Inn Fields; when a very satisfactory report upon the state of the Society's affairs and proceedings during the last year was read, and ordered to be printed. This report paid a due tribute of respect to several members whom the Society has lost by death in the last year, and particularly to Col. W. Lambton, of Madras, and Dr. Walbeck of the Observatory at Abö. It gives a succinct account of the measurement of the largest continuous arc of a meridian yet effected, which occupied the former gentleman upwards of twenty years in India.

The Chairman (Mr. Colebrooke) then proceeded to distribute the honorary rewards of the Society, viz. the Society's gold medal to Charles Babbage, Esq. FRS. as a token of the high estimation in which it holds his valuable invention of an engine for calculating mathematical and astronomical tables, being the first medal awarded by the Society. A similar gold medal to Prof. J. F. Encke, Director of the Observatory at Seeberg, in Gotha, for his investigations relative to the comet which bears his name, and which led to the rediscovery of it in 1822. The silver medal of the Society to Prof. Charles Rumker for the rediscovery of Encke's comet, and a similar medal to M. Jean Louis Pons, of La Marlia, in Italy, for the discovery of two comets on the 31st of May and 13th July, 1822, and for his indefatigable assiduity in that department of astronomy. The chairman prefaced the presentation of each medal by a most eloquent, learned, and interesting address of considerable length, all of which were delivered in the most impressive manner. They were replete with information on the successive improvements in machinery for assisting calculation, as well as on cometary astronomy, and we were happy to find, that in consequence of a motion made by Davies Gilbert, Esq. MP. and seconded by John Fuller, Esq. they will be printed for circulation among the members.

The election for the Officers and Council of the Society for the ensuing year then took place, when the following appeared to be the unanimous choice of the meeting, viz.:—

President.—H. T. Colebrooke, Esq. FRSL. and E. and FLS.

Vice-Presidents.—C. Babbage, Esq. MA. FRSL. and E.; F. Baily, Esq. FRS. and LS.; Sir B. Hobhouse, Bart. FRS.; and The Right Hon. George Earl of Macclesfield, FRS.

Treasurer.—Rev. W. Pearson, LLD. FRS.

Secretaries.—O. G. Gregory, LLD. Prof. Math. Roy. Mil. Acad. Woolwich; J. Millington, Esq. FLS. Prof. Mech. Philos. Royal Institution.

Foreign Secretary.—J. F. W. Herschel, Esq. MA. FRSL. and E.

Council.—Major T. Colby, Roy. Eng. LLD. FRSL. and E.; G. Dollond, Esq. FRS.; Bryan Donkin, Esq.; Capt. J. Franklin, RN. FRS.; D. Gilbert, Esq. MP. VPRS. FLS.; B. Gompertz, Esq. FRS.; S. Groombridge, Esq. FRS.; and D. Moore, Esq. FRS. SA. and LS.

Several new members and associates were nominated, and the greater part of the Society adjourned to Freemasons' Tavern, where a dinner was provided.

GEOLOGICAL SOCIETY.

Dec. 19, 1823.—A paper was read, containing Geological Observations collected on a Journey through Persia from Bushire in the Persian Gulph to Teheran; by James B. Fraser, Esq. MGS.

The author is of opinion that both the east and west sides of the Persian Gulph to a great extent, consist of a calcareous formation, which, it is ascertained, in many parts continues far inland. In a part of this formation, his route from Bushire commenced; between which place and Shiraz, the hills are composed of sulphates and carbonates of lime, and the strata often much disturbed. Through a large tract of this country, carbonate of lime is intermixed with the gypsum, but in parts, rocks of pure gypsum occur, and very frequently accompanied by salt. Streams and lakes of salt abound, and there is a considerable one of the latter at Shiraz. Proceeding northward, the route from Shiraz to Ispahan, a distance of about 250 miles, lies over an elevated country, the nature of which is similar to that before described, but the carbonate of lime predominates. Between the village of Gendoo and the town of Yes-dikhaust, Mr. Fraser found clay slate, and a conglomerate rock inclosing pebbles of quartz, greenstone, and limestone, cemented by carbonate of lime; strata of this aggregate rock alternate with a finer sandstone. The mountains between Ispahan and Teheran are of a character very different from the preceding; among them clay slate was observed, and the highest region, which reaches a great elevation, consists of granitic rocks.

Jan. 2, 1824.—A paper was read "On the Geological Structure of St. Jago, one of the Cape de Verd Islands; by Major Colebrooke."

From the observations of the author, and the accompanying specimens, it appears that at the landing place near the town of Porto Prago, in St. Jago, the rock of the cliff is composed of fragments of trap imbedded in a hard pure white carbonate of lime. The fragments of this breccia are generally small, and none of them rounded by attrition. The cliff on which stand the batteries and town of Prago, is regularly stratified, and at the bottom are beds of a calcareous sandstone alternating with others which contain specimens of a large oyster; in both of these beds occur pebbles of trap. The stratum which crowns the cliff is from eight to twelve feet in thickness, and consists of trap.

Jan. 16.—A paper entitled "Outline of the Geology of the South of Russia; by the Hon. William T. H. Fox Strangways, MGS." was read in part.

Feb. 6.—On this day, being the anniversary of the Society, the following gentlemen were chosen as Officers and Council for the year:—

President.—Rev. W. Buckland, FRS. Prof. Geol. and Min. Oxford.

Vice-Presidents.—A. Aikin, Esq. FLS.; J. Bostock, MD. FR. and LS.; G. B. Greenough, Esq. FR. and LS.; and H. Warburton, Esq. FRS.

Secretaries.—C. Lyell, Esq. FLS.; P. B. Webb, Esq. FLS.; and T. Webster, Esq.

Foreign Secretary.—H. Heuland, Esq.

Treasurer.—J. Taylor, Esq.

Council.—Sir T. D. Acland, Bart. MP.; J. Duke of Bedford, FL. and HS.; W. Clift, Esq. FRS.; H. T. Colebrooke, Esq. FR. and LS.; Major T. Colby, LLD. FRS. L. and E.; T. Horsfield, MD. FLS.; Sir A. Crichton, MD. FR. and LS.; C. Stokes, Esq. FRA. and LS.; T. Smith, Esq. FR. and LS.; W. H. Pepys, Esq. FR. L. and HS.; Rev. Adam Sedgwick, MA. FRS. Woodwardian Prof. Cambridge; W. H. Fitton, MD. FRS.

Keeper of the Museum and Draughtsman.—T. Webster, Esq.

Feb. 20.—A notice was read, of the Discovery of a perfect Skeleton of the Fossil Genus hitherto called Plesiosaurus; by the Rev. W. D. Conybeare, FRS. MGS.

The Plesiosaurus, which is the subject of this notice, was found in the blue lias of Lyme Regis, in Dorsetshire. In the whole exterior portion of its vertebral column the skeleton is entire, and of the remaining parts of the animal few are wanting. In the Transactions of the Geological Society, vol. 5, and vol. 1 second series, the author had attempted to assign to the various dispersed and disjointed remains of this animal which were then known, their relative places in the skeleton, and his opinions, he observes, have now, in all essential points, received full confirmation. After pointing out the errors into which he had fallen, Mr. Conybeare describes the osteology of this remarkable fossil animal; the most characteristic and distinguishing features of which are, the extraordinary length of the neck, which fully equals that of the body and tail united, and the number of its vertebræ, which very far exceeds that of any animal previously known.

METEOROLOGICAL SOCIETY.

Jan. 14.—The Committee appointed by the Council of this Society to consider and report upon the best means of establishing correct and complete series of meteorological observations having presented their *Preliminary Report*, it was communicated by the Council to the Society, at this meeting.

The Committee represent in this Report, that the first and principal object of the Society is to obtain accurate and comparable observations of atmospheric phænomena from all parts of the world; and after adverting to the advantages which would result from the general adoption by meteorologists of instruments graduated upon the same scales, to be examined by all at the same hours, and the results noted upon the same plan, they proceed as follows:

"The Committee are, however, aware, that there are many and weighty difficulties in the way of such an arrangement, and that much time would necessarily be required for their consideration and discussion; they therefore recommend that immediate measures be taken to procure correct registers of comparable observations from different parts of Great Britain and its colonies, as well as from other parts of the world, with instruments graduated to the common scales. To effectuate this purpose with advantage, they consider it absolutely necessary that the Meteorological Society of London should set the example of the requisite precision by establishing a Meteorological Observatory in the metropolis, or its vicinity, and instituting operations to be conducted with undeniable accuracy, and with instruments of standard excellence."

A paper was read "On the Natural History and probable Causes of the Vernal Winds of the North of England, as they prevail in Westmoreland; by John Gough, Esq. of Kendal. Communicated by Dr. Birkbeck, President of this Society."

In this paper, Mr. Gough minutely describes the phænomena which accompany and characterise the periodical easterly winds of spring, especially as they prevail in Westmoreland, and states the following opinion respecting their cause, with various illustrations of it. The cause of these winds may be referred to the progressive advances of the spring from the south to the north. This season commences in Italy about the 20th of February; it is equally advanced in Westmoreland about the middle of April, at which time the countries situated on the confines of the Arctic Circle remain buried in snow. This covering will unavoidably arrest the progress of spring in its advances towards the Arctic Circle, and prolong a milder kind of winter in the northern regions. The delay here pointed out is certain and annual, because the solar heat, instead of warming the surface of the country thus buried in snow, is absorbed by the icy covering, and employed in converting it into water of the temperature of melting ice. While the sun is employed in removing this impediment to vegetation in the north, his beams are warming the plains and valleys of England, in consequence of which the thermometer in the shade frequently stands between 60° and 70° at noon, during the latter part of April, and falls occasionally to the freezing point in the night.

These facts show that the inhabitants of Britain enjoy an advanced state of temperature, while the people of Sweden and Norway are exposed to a degree of cold equal to the rigours of our winter. The preceding difference in the temperature of the atmosphere of Britain and the more northern regions gives a greater specific gravity to the air of Sweden and Norway than to that of England, as well as all the intervening countries that are free from snow; and this excess of density is, in Mr. Gough's opinion, the cause of the vernal winds.

Several other communications were likewise read.

MEDICO-BOTANICAL SOCIETY.

Jan. 15.—The Medico-Botanical Society of London held their anniversary meeting, when the following Council was elected for the ensuing year, viz. :—

Dr. Bree, FRS. *President*; Dr. Paris, FRS.; Dr. E. T. Monro; J. Brookes, Esq. FRS.; W. T. Brande, Esq. FRS.; Sir J. Mac Gregor, MD. FRS.; Sir A. Crichton, MD. FRS. *Vice-Presidents*; W. Newman, Esq. *Treasurer*; Mr. C. Holdstock, *Secretary*; J. Frost, Esq. *Prof. of Botany*; T. Jones, Esq.; W. Yarrell, Esq.; T. Andrews, Esq.; A. White, Esq.; Dr. J. Elliotson.

Article XVII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Improvement in the Mountain Barometer.*

Mr. Newman has proposed to remedy the inconveniences of the common instrument: what these are, and the mode of removing them, we shall give in his own words:—

"The object has been to correct those defects and errors which arise from the use of a wooden cistern and leather bag, in the common barometer. It has been found that when the cistern is made of a wood sufficiently sound and close-grained to permit of the pressure required from the screw to make the instrument portable, that it is so impervious to air, as not to allow it to pass with sufficient freedom, and consequently, that when the instrument is used at any great altitude, the mercury cannot fall into the cistern except with considerable difficulty, and a long time is required before an accurate observation of the air's pressure can be made; most generally, however, the cistern is sufficiently pervious to air, but it is then found that on screwing up the mercury to the top of the tube, a portion of the metal generally makes its way through the wood, thus soon rendering the instrument quite useless; for it is very evident that a barometer that loses a portion of mercury from the cistern by making it portable or otherwise after it is adjusted, can no longer be correct, or give the height of the column.

"To obviate these inconveniences, I have substituted a cistern of iron in place of the wooden one; it is fastened to the tube by a thick collar of wood, which is glued on in the usual manner; a screw passes through the centre of the bottom, so as to move in a line with the barometer tube; it is terminated inside the cistern by a piece of cork tied over with leather, so that the instrument being inclined that the tube may be filled with mercury, this cork may be screwed up against the end of the tube, and effectually preserve the metal within from oscillation, without subjecting the cistern itself to any pressure.

"As there is no pressure on the mercury in the cistern, the wooden cap may be left so porous in one part, as to allow of the ready access of air, so that the column shall fall freely to its proper level, without any danger of losing mercury.

"Another great object in a mountain barometer, is to obtain the temperature of the mercury, which is done by fixing a thermometer with the bulb in the cistern; I have found that by carrying a barometer in my hand and near the body, the temperature is increased considerably, and will frequently rise as high as 85° Fahr.

"In the barometer of common construction, the height of the column of mercury is marked off from another instrument, presumed as a standard, and in that case, the actual height is rarely or ever given, for every change that takes place in the weight of the atmosphere, alters barometers more or less according to the proportion which the diameters of the tubes bear to those of the cisterns, and for that reason, upon examining twenty barometers no two will agree, unless they were marked off together, and happen to stand at that exact height.

"To remedy this source of error each instrument may be reckoned a standard, the height of the column is marked off from the surface of the mercury, and the point given at which it was marked off; when with the correction for the capacities of the tube and cistern, and also the temperature, the actual height of the barometer is ascertained. Upon examining the first four which I made independent of each other on this principle, one for Mr. Daniell, one for the Royal Society, and two for Capt. Sabine, they agreed within ·004 of an inch with each other."—(Institution Journal, xvi. 277.)

II. *Vegetable Alkalies.*

Mr. Brande has lately given analyses of several of these bodies, some of which differ much from those of MM. Dumas and Pelletier, mentioned in the *Annals* for January. The most remarkable discrepance is in the analysis of cinchonia. The mean of Mr. Brande's analyses gives as its constituents

Carbon	79·30
Azote	13·72
Hydrogen	7·17
	100·19

This substance, according to MM. Dumas and Pelletier, contains oxygen, and consists of

Carbon	76·97
Azote	9·02
Hydrogen	6·22
Oxygen	7·79
	100·00

The following, Mr. Brande states as an approximation only to the correct proportions of the elements of quina:

Carbon	73·80
Azote	13·00
Hydrogen	7·65
Oxygen	5·55
	100·00

MM. Dumas and Pelletier find its constituents to be:

Carbon	75·02
Azote	8·45
Hydrogen	6·66
Oxygen	10·43
	100·56

The differences in the proportions of azote and oxygen are very considerable.

In the analysis of morphia, the agreement is much greater than could have been expected from the great differences noticed in cinchonia and quina. Mr. Brande gives as the elements,

Carbon	72·0
Azote	5·5
Hydrogen	5·5
Oxygen	17·0
	100·0

while MM. Dumas and Pelletier state its composition to be:

Carbon	72·02
Azote	5·53
Hydrogen	7·01
Oxygen	14·84
	99·40

According to Mr. Bussey (*Annals*, N. S. vi. 229), morphia is composed of

Carbon	69·0
Azote	4·5
Hydrogen	6·5
Oxygen	20·0
	100·0

In this analysis there is a difference of more than five per cent. in the quantity of oxygen compared with that in the last quoted; but while MM. Dumas and Pelletier differ from Mr. Brande in assigning nearly eight per cent. of oxygen to cinchonia, they and M. Bussey differ from Dr. Thomson in stating azote to be one of the constituents of morphia, and, according to him, it is composed of

Carbon	44·72
Hydrogen	5·59
Oxygen	49·69
	100·00

From the different statements which have been thus made with respect to the composition of these vegetable alkalies, it appears proble, either that different substances have been employed under the same name, or that impurity must have been in some cases mixed with them; if the difference existed as to the quantity of any other element instead of the presence of azote, it might, perhaps, be referred to moisture, or water of crystallization; but as the case stands, further experiments are necessary.

III. *Dœbereiner's Eudiometer.*

Prof. Dœbereiner having suggested the use of finely divided platina for the purpose of detecting minute portions of oxygen in a gaseous mixture, in which hydrogen also is present, Messrs. Daniell and Children mixed 20 measures of atmospheric air, with 37 measures of hydrogen gas, and passed up to the mixture a small portion of the platina powder, procured by heating the ammonia muriate to redness, and made into a ball with precipitated alumina. The pellet was heated red by the blowpipe, immediately before it was used; its size about that of a small pea. The absorption amounted to 13 measures = 4·3 oxygen, being 0·1 of a measure more than the quantity of oxygen in 20 measures of atmospheric air, which may probably have arisen from a slight impurity in the hydrogen, or from some minute unperceived bubbles of air, entangled in the mercury.

Another mixture of common air and hydrogen, in which the latter was in considerable excess, was deprived of its oxygen by the pellets, and when the absorption was complete, 38 measures of the residual gas were taken, and a fresh pellet, heated to redness, immediately before it was used, passed up. After standing about a quarter of an hour, no absorption had taken place. The tube and the mercury were then placed before the fire, till the whole apparatus was too hot to be touched with the naked hand. It was then removed from the fire, and when cooled to its original temperature, the mixture occupied, as before, exactly 38 measures. The powder of platina with hydrogen seems, therefore, to be admirably calculated for eudiometrical purposes. Its application is extremely simple and easy, it is speedy in its effect, and no error need be apprehended from the formation of ammonia, even at considerably elevated temperatures. It appears also to be well calculated for ascertaining the purity of simple gases, at least as far as regards admixture of atmospheric air. The oxygen of a very minute portion of common air, mixed with carbonic acid gas, and a little hydrogen, was immediately absorbed, on passing up one of the little pellets to the mixture.—(Institution Journal, xvi. 374.)

IV. *New Minerals.*

Mr. Brooke has lately described two new mineral bodies; to the first he has given the name of *Childrenite*, on account of the attention, among other inducements, which Mr. Children has shown to mineralogical chemistry. This mineral was met with in Devonshire, and was said to have been taken from some part of the ground which had been perforated for the Tavistock canal; it was supposed at first to be carbonate of iron; but Dr. Wollaston determined that it was a *phosphate of alumina and iron.*

The primary form of the crystal is assumed by Mr. Brooke to be a *right rhombic prism;* but he has not succeeded in cleaving it. The crystals scratch glass; their colour is wine-yellow, they occur on the surface of crystallized quartz, and might be mistaken by a casual observer for sulphate of barytes.

The next mineral was sent, among other Vesuvian substances, to Mr. Brooke by Dr. Somerville, from which circumstance he has named it *Somervillite;* the primary form of the crystal is *a right square prism,* but the crystals are modified by numerous secondary planes; they may be cleaved parallel to the terminal planes, but imperfectly, if at all, parallel to the lateral planes, or to the diagonals of the prism; their colour is a very pale dull-yellow: they occur in cavities, with crystallized black mica, and with another substance not yet examined; the mass to which they adhere appears to be nearly all Somervillite, intermingled with black mica.

This substance might at first view be mistaken for idocrase; but it is much softer: the cleavage parallel to the terminal planes much more distinct, and the cross fracture more glossy. Mr. Children has also compared the characters of this mineral under the blowpipe with those of idocrase. When exposed alone in the forceps, it slightly decrepitates, which idocrase does not, and fuses, with greater difficulty than idocrase, into a greyish glass, the globule from idocrase being greenish. With borax, in the reducing flame, idocrase produces a light-green, and this a colourless glass.

Mr. Brooke has likewise examined the mineral called *kupferschaum* by the Germans, of which he had not met with any analysis. This mineral, which is the same as the fibrous or flaky bright-green substance found at Matlock, appears to be a *carbonate of copper and zinc*.—(Institution Journal, xvi. 274.)

V. *Death of Mr. Bowdich, in Africa.*

It is with unfeigned regret that we announce the death of this enterprising and accomplished traveller: he has fallen a victim to his over exertions in surveying St. Mary's River, in Gambia: he expired, after lingering a fortnight, on the 10th of January, leaving a widow and three children totally unprovided for. We are happy to learn that proposals will shortly be issued for publishing, for their benefit, Mr. Bowdich's "Excursions in Madeira and Porto Santo," a work which he had completed prior to his decease.

Article XVIII.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Elements of Physiology, by J. Bostock, MD. is nearly ready.

Capt. Sir Henry Heathcote, RN. has in the press, a Treatise on Staysails for the Purpose of intercepting Wind between the Square Sails of Ships and other Square-rigged Vessels: illustrated by suitable Diagrams, and Plates.

A Treatise on the Nature, Symptoms, and Cure of Cataract; by John Stevenson, FRCS. 8vo.

A Treatise on Mineralogy, by Frederick Mohs; translated from the German by W. Haidinger. 2 vols. post 8vo.

Extracts from a Journal written on the Coasts of Chili, Peru, and Mexico, in the Years 1820, 1821, and 1822; by Capt. Basil Hall, RN. Author of a Voyage to Loo Choo. 2 vols. post 8vo.

JUST PUBLISHED.

A Second Letter to Sir John Cox Hippesley, on the Mischiefs incidental to the Tread-Wheel, as an Instrument of Prison Discipline, containing an Examination of the Official Reports upon this Subject, returned to the Secretary of State's Office, during each Session of Parliament. By John Mason Good, MD. FRS. &c. Price 2*s*. 6*d*.

The West India Colonies: the Calumnies and Misrepresentations circulated against them by the Edinburgh Review, Mr. Clarkson, Mr. Cropper, &c. examined and refuted. By James Mac Queen. 8vo. 12*s*.

The Pupil's Pharmacopœia; being a literal Translation of the New Edition of the London Pharmacopœia, &c. By W. Maugham, surgeon. 6*s*.

Sketches of the Philosophy of Apparitions; or an Attempt to trace such Illusions to their Physical Causes. By S. Hibbert, MD. FRSE. &c. 10*s*. 6*d*.

The English Flora. By Sir James E. Smith, President of the Linnean Society, &c. Vols. 1 and 2. 1*l*. 4*s*.

The New Pharmacopœia of the Royal College of Physicians of London, 1824; translated by Sir George L. Tuthill, Kt. MD. FRS. 8vo. 7*s*. 18mo. 4*s*.

Gardner on Iodine. 8vo. 4*s*.

Article XIX.

NEW PATENTS.

G. Pollard, Rupert-street, St. James, Middlesex, brass-founder, for certain improvements on machines for levigating or grinding colours used in the various branches of painting, which machinery may be worked by any suitable power, and is applicable to other useful purposes.—Jan. 19.

J. Russel, Wednesbury, Staffordshire, gas-tube manufacturer, for his improvement in the manufacture of tubes for gas and other purposes.—Jan. 19.

S. Broadmeadow, Abergavenny, Monmouthshire, civil engineer, for his improved method of manufacturing and purifying inflammable gases by the admission and admixture of atmospheric air.—Jan. 19.

H. Fletcher, Walsall, Staffordshire, saddler's ironmonger, for certain improvements in tanning hides and other skins.—Jan. 19.

T. Bewley, Mount Rath, in Queen's County, Ireland, cotton manufacturer, for certain improvements in wheeled carriages.—Jan. 24.

J. Heathcoat, Tiverton, Devonshire, lace manufacturer, for certain improvements in the method of figuring or ornamenting various kinds of goods manufactured from silk, cotton, or flax.—Jan. 24.

J. Jones, Leeds, Yorkshire, brush manufacturer, for certain improvements in machinery and instruments for dressing and cleansing woollen, cotton, linen, silk, and other cloths or fabrics.—Jan. 27.

Sir W. Congreve, Bart. Cecil-street, Strand, for his improved method of stamping.—Feb. 7.

ARTICLE XX.

METEOROLOGICAL TABLE.

1824.	Wind.	Barometer.		Thermometer.		Evap.	Rain.
		Max.	Min.	Max.	Min.		
2d Mon.							
Feb. 1	S E	30·20	30·11	45	25	—	
2	E	30·27	30·20	43	24	—	
3	S E	30·20	29·89	45	32	—	07
4	W	29·97	29·89	49	35	—	16
5	W	30·20	29·97	43	32	—	
6	N W	30·29	30·20	45	32	—	—
7	S W	30·37	30·29	49	45	—	—
8	W	30·50	30·37	54	48	—	—
9	Var.	30·50	30·48	52	35	—	—
10	S W	30·48	30·44	51	31	—	—
11	N W	30·44	30·16	46	32	—	—
12	S W	30.16	29·42	45	35	·45	12
13	W	29·42	29·05	48	37	—	1·04
14	W	29·60	29·05	45	32	—	02
15	N E	29·75	29·60	42	26	—	
16	N	29·75	29·61	43	28	—	
17	N E	29·61	29·50	42	29	—	
18	E	29·57	29·50	45	34	—	06
19	E	29·66	29·57	45	37	—	10
20	E	29·91	29·66	47	36	—	33
21	S W	30·04	29·91	42	30	—	
22	E	30·12	30·04	47	39	—	08
23	E	30·12	30·12	42	38	—	
24	E	30·12	29·98	43	27	—	
25	E	29·98	29·96	38	30	—	
26	N	29·96	29·90	43	30	—	
27	N	29·97	29·90	39	33	—	24
28	S W	30·05	29·97	42	34	—	10
29	N	29·97	29·94	45	34	·40	
		30·50	29·05	54	24	0·85	2·31

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Second Month.—1. Clear morning with white frost: fine day. 2. Very fine day. 3. Hoar frost: foggy. 4. Cloudy. 5. Fine: lunar halo at night. 6. Ditto. 7. Drizzling. 8. Overcast. 9, 10. Drizzly. 11. Very fine morning. 12. Drizzly. 13. Morning fine: afternoon rainy: night stormy. 14. Cloudy. 15—17. Fine. 18. Cloudy and fine: a little snow in the morning between five and six, which melted immediately. 19. Fine. 20. Drizzly morning: rainy evening. 21. Drizzly: very foggy night. 22. Foggy morning: fine afternoon. 23. Bleak. 24, 25. Fine. 26. Bleak: some hail and snow at three, p. m. 27. Rainy: some hail at half-past eight, a. m. 28. Cloudy: rainy evening. 29. Cloudy.

RESULTS.

Winds: N, 4; E, 8; SW, 5; SE, 2; W, 5; NW, 2; NE, 2; Var. 1.

Barometer: Mean height

For the month.................................. 29·963 inches.
For the lunar period, ending the 20th.................. 29·929
For 14 days, ending the 2d (moon south) .... 29·938
For 13 days, ending the 15th (moon north) .... 30·026

Thermometer: Mean height

For the month.................................. 39·051°
For the lunar period.............................. 40·017
For days, the sun in Aquarius.................. 40·216

Evaporation.. 0·85 in.

Rain.. 2·31

Laboratory, Stratford, Third Month, 24, 1824. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

MAY, 1824.

ARTICLE I.

Remarks on Solar Light and Heat. By Baden Powell, MA. of Oriel College, Oxford.

(To the Editor of the *Annals of Philosophy.*)

SIR, *April* 2, 1824.

IN explanation of the design of the present communication, I conceive it necessary to observe in the first instance, that having been engaged in experiments on solar light and heat, I have laid accounts of some of them before the Royal Society (see reports of Royal Society, *Annals*, Feb. 1824): those accounts, however, being confined to the mere detail of the experiments, I wish through the medium of your journal to offer some remarks on the subject, of a more general nature, and which may be considered as forming a sort of introduction to such experimental researches.

If then in taking a brief review of the present state of our knowledge upon this subject, my remarks and statements should not be of a nature wholly new, my design as thus explained will be a sufficient excuse; and the more so as I could not proceed to the few experiments here given without such preliminary considerations. I am, Sir, your obedient servant,

B. POWELL.

I. (1.) Speaking according to our ordinary sensations, we are accustomed to say that the sun communicates both light and heat. Light is transmitted in a way which we term radiation. The heat from non-luminous hot bodies is transmitted to a distance in a way closely analogous; and to which the same name has been applied. In the first instance we might suppose that the sun sends out two separate emanations, one of light, and another distinct from it, and similar to that of radiant heat from a mass of hot water; and this, perhaps, was the first view taken of the subject, though a confused idea of some very close and intimate connexion subsisting between the solar light and heat appears to have prevailed.

(2.) This subject, as might naturally be expected, attracted the early notice of experimenters. A very slight examination sufficed to show that the rays of solar heat (whatever their nature might be) differed essentially in many properties from those of terrestrial heat, whether radiated from luminous or non-luminous bodies. Whether there existed a separate set of heating rays distinct from those of light, and at the same time differing in many respects from rays of terrestrial heat; or whether these differences depended on some unknown property of the rays of light, was a question which for a long time remained without any direct investigation, and on which even now, we have, perhaps, no very precise ideas.

Among the earliest experiments on the subject, if not actually the first, were those of Mr. Boyle, on the different degrees of heat communicated by the sun to black, white, and red-coloured surfaces. These were extended and confirmed in the well-known investigations of Dr. Franklin, &c.

"Mr. Boyle caused a large block of black marble to be ground into the form of a spherical concave speculum, and found that the sun's rays reflected from it were far from being too powerful for his eyes, as would have been the case had it been of any other colour; and although its size was considerable, yet he could not set a piece of wood on fire with it; whereas a far less speculum of the same form, made out of a more reflecting substance, would presently have made it flame."—(Boyle on Colours, &c.)

Scheele conceived that the sun's rays of light produced heat, not when *in motion*, but only when *stopped* by the *interposition* of *solid* bodies.—(Treatise on Air and Fire, &c.)

Mr. Melville seems to have viewed the matter nearly in the same light, and to have conceived *reflexion* at an opaque surface, the cause of excitation of heat from the sun's rays.—(See Phil. Mag. June, 1815, a paper by Dr. Evans.)

(3.) In later times the experiments of Prof. Leslie, Sir H. Davy, &c., have sufficiently established the property possessed by that emanation (whatever its nature may be, whether simple or compound) which is derived from the sun, of producing greater heat in bodies in proportion as their surfaces owing to darkness of colour, have the capacity of absorbing rays of light.

It has been equally well established by Prof. Leslie, Count Rumford, &c. that the heat emanating from a mass of non-luminous hot matter, has no such relation to the colour, though a very close one to the nature and texture of the surface.

(4.) The experiments of Sir E. Home (Phil. Trans. 1821, Part I.) are particularly deserving of attention, as exhibiting what might at first sight be considered an exception to the above remarks; a greater effect being produced in some instances on a white than on a black surface. A more attentive examination, however, will show us that these experiments prove thus much. The heat occasioned by the rays of the sun when received directly, or when in some degree intercepted as by thin white cloth, on the skin, is greater than that communicated by conduction to the same skin, through a black cloth in contact with it, which is itself, in the first instance, heated by absorbing the rays.

A white skin is scorched, and a negro's skin is not, in ten minutes by the direct rays of the sun; that is, as before, the outer coat of the white skin allows some of the direct rays to pass through and affect the sentient substance beneath; whereas in the case of the black skin, the rays are absorbed by the black surface, and so affect the sentient parts only as heat of temperature.

II. (5.) As to the nature of this heating effect, the greatest difference of opinion has long prevailed among the most distinguished philosophers; one party maintaining the totally distinct existence of light and radiant heat in the compound solar beam; the other contending for the absolute identity of the two: the same principle being merely displayed under two different modifications. (See Sir W. Herschel, Phil. Trans. 1800, Part II.; Leslie on Heat, p. 162, Biot, Traité de Physique, vol. iv. p. 690, &c.)

Without entering upon an examination of the merits of either theory, we may proceed to remark that the first object in the inductive examination of this subject is to ascertain distinctly what peculiar properties of this heating emanation we can fix upon by which its nature may be defined, and by the help of which we may be enabled to compare it with other heating emanations.

(6.) Among the most obvious properties of the solar rays, we perceive that before adverted to, viz. that they produce heat on bodies in proportion to the darkness of their colour, and not in regard to the absorptive qualities of the texture of their surfaces for the heat from non-luminous bodies. This relation is universal, and without any exceptions; it is consequently one which we can satisfactorily adopt as the foundation of a distinctive description.

(7.) We may from this advance to another test, which will afford an additional characteristic. It has been distinctly shown that all heating emanations from terrestrial bodies whether luminous or not, are more or less stopped, or even in some cases totally intercepted, by the interposition of a glass screen. Similar experiments may easily be tried on the solar rays.

That little or no diminution of effect is produced on a blackened thermometer exposed to the sun, by the interposition of glass, has been shown by several experiments. As it is remarked by De la Roche (Biot, Traité de Phys. vol. iv. p. 611), I have also frequently observed the same thing, taking notice of the temperature of the glass, as will be subsequently seen. But there is another part of the question which still appears to me to want further examination. The sun's rays produce some heating effect on surfaces of a light colour. I have, therefore, tried whether also in this case, and when the texture of the surface was very absorptive for simple radiant heat, a glass screen has any power to diminish the effect. Two thermometers were exposed together to the direct and screened rays, one having its bulb coated with indian ink; the other with a thin paste of chalk and water; the bulbs were free from contact. If there existed in the solar beam any rays of such a nature that they were affected by the *texture* rather than the *colour* of surfaces, and were *not* capable of *passing* through *glass*, they would be affected by a surface of chalk more than one of indian ink. If they formed only a small proportion of the whole, the diminution, when glass was interposed before the *inked* thermometer, might be so small as to be imperceptible; but with the *whitened* surface, it would be much more conspicuous.

(8.) The following are the results of two sets of experiments conducted on this principle:

	Thermometers.				Temp. of glass.		Thickness of glass.
	Exposed.		Screened.				
Rise in two minutes centigrade.	A. White.	B. Black.	A. White.	B. Black.	Before exper.	After exper.	Inch.
	4·0°	6·5°	5·0°	9·0°	18·0°	19·0°	0·44
	4·0	5·5	4·0	7·0	20·5	20·5	—
	4·5	7·0	3·5	6·5	21·0	21·0	—
	5·5	8·0	4·5	7·5	17·0	17·5	—
	5·0	9·0	2·5	3·5	—	—	—
	3·5	7·0	6·0	8·5	—	—	—
	1·5	3·0	3·5	7·0	18·5	19·0	0·06
Mean	4·0	6·57	4·14	7·0			

	THERMOMETERS.						
	Exposed.		Screened.		Temp. of glass.		Thickness of glass.
	B. White.	A. Black.	B. White.	A. Black.	Before exper.	After exper.	Inch.
Rise in two minutes centigrade.	4·5°	7	5·0	7	18	20·0	0·06
	3·25	6	4·0	6	20	20·5	—
	5·0	10	5·0	10	19	19·0	0·44
	4·0	7	3·25	7	19	19·0	—
Mean	4·18	7·5	4·31	7·5			

(9.) These results exhibit a very close agreement in the ratio of the risings of the two thermometers, when exposed, and when screened, and this with glasses of different thickness, at different times, and with different absolute intensities of the sun's rays; as also when the colours of the bulbs were mutually changed. The mass of the bulb A was somewhat greater than B; the glass acquired no heat sufficient to interfere with the results; and the thermometer was always placed so that the bulbs were not near any object which might radiate heat. The temperature of the air affecting both *surfaces* equally would tend to diminish the ratio. To its variation, I conceive, the trifling difference in the ratios may fairly be ascribed.

(10.) Hence, I think, we are entitled to conclude, that there do not exist in the solar beam in its natural state any rays of the description just alluded to; but that the whole emanation consists of one sort of rays distinguished by the two characteristics of affecting substances with heat in proportion to the *darkness* of their colour, and being wholly *transmissible* through *glass* without heating it; and that these same rays when they infringe on the eye are capable of producing the sensation of vision; and by the absorption of some, and the reflexion of others, of their constituent parts, at the surfaces of bodies, produce the phenomena of colours.

(11.) The heating effect maintains an intimate relation to light both in respect to the substances which it traverses without interception, and to those by which it is absorbed, and to the degree of absorption. It is found to accompany the rays of light in the most constant and inseparable manner: through whatever substance, and in whatever direction it takes its course, this is strikingly exemplified in one of Sir W. Herschel's experiments (Phil. Trans. 1800, No. 13, Exper. 11), in which the heating effect is shown to accompany the rays of light in all the alterations of its course through a Newtonian telescope with four lenses.

Speaking in general terms, within ordinary limits, and for light of the same colour, we may say, that the *heating* effect increases or decreases in proportion to the intensity of illuminating effect. Prof. Leslie considers the proportion to be precise and undeviating.

(12.) Whatever we suppose to be the state in which the heat exists when it thus inseparably accompanies the luminous rays, it is evident that there must be some peculiar circumstance in the mode of its union which makes its effects sensible only under some particular circumstances; and under others endows it with properties which heat in its simple radiant state does not possess. It evidently exists in a state essentially different from that of simple radiant heat; and we may in general say, that it is never developed or rendered sensible except under such circumstances as produce at the same time some modification or change in the light itself.

Upon considering all these well established facts, I think, instead of using such terms as "calorific rays," and "luminous rays," it is much more conformable to facts, and involves no hypothetical ideas, to describe the phenomena by the terms "rays of *light*," and the "*heating power* or *property*" of those rays.

III. (13.) Thus far my remarks have been confined to the nature of the heating power of the sun when its rays are in that state in which they *naturally* are, as coming directly from that luminary. In the next place we have to inquire whether by any modification which these rays may be made to undergo by *artificial* means, we can attain to any further elucidation of the nature of this heating power.

(14.) It has been shown by the experiments of M. Berard (Biot, Traité de Phys. vol. iv. p. 603, &c.), that when light undergoes polarization, the heating power participates in that effect.

It has also been shown by the same distinguished philosopher (see Biot as above), that simple radiant heat when unaccompanied by light is susceptible of being polarized also. In considering these results, we must be careful not to confound them together; because simple rays of heat are capable of displaying the effects of polarization, and the heating effect in the solar beam also obeys the same impulse, we must not hastily conclude that the same agent existing in the same form is, therefore, the common heating principle in both cases.

(15.) The heating power of the sun is well known to be capable of being collected in a focus along with the luminous rays, both by reflexion and refraction. By the former means simple radiant heat may also be concentrated: this circumstance again shows a *similarity*, but does not prove an *identity* in the agents or powers.

In one of Sir W. Herschel's experiments (Phil. Trans. 1800, No. 15, Exper. 23), a focus of heat different from that of light seems to be proved. This opinion it is not my design at present either to maintain or controvert. I have only to observe, that granting its truth, it must not be applied as an argument to show that simple radiant heat exists distinctly in the solar beam; or in other words, that the sun's heat is produced by an agent identical with that which emanates from non-luminous hot bodies; it merely shows that the refractive and dispersive power which the lens exercises on *light* is capable of *eliciting from it* a certain *agent* or *set of rays* which produces the sensation of heat, but not that of illumination; and by no means shows that that agent exists in a separate form when the rays of light are in their direct natural condition.

If the focus of heat be the same as that of light, and the experiment of Sir W. Herschel (Phil. Trans. 1800, No. 15, Exper. 19, 20) be admitted, where the concentration of simple radiant heat by a lens appears to be proved, the same remark applies as that just made with respect to concentration by a mirror.

(16.) The principal modification which the sun's rays are made to undergo, and from which conclusions relative to the nature of their heating power have been drawn, is the analysis to which they may be subjected by the prism. Into any discussion upon the controverted points respecting these experiments, I shall abstain from entering. We will suppose it granted that a set of invisible heating rays are separated beyond the visible red rays. The existence of such rays in a distinct state in the spectrum cannot be considered as any proof that they have that *distinct* existence in the natural state of the rays. It by no means proves that any such simple heating rays must have come directly from the sun, and have been transmitted through the prism, with merely a change in their direction.

From the experiments above given, we may, in reference to this point learn thus much: the direct rays are not accompanied by any separate heating rays which are either stopped by glass, or bear a relation to texture more than colour. It therefore becomes an important object to try whether in the prismatic beam these heating rays possess those characteristics or not. With respect to one of the characteristics, viz. transmissibility through glass, we have no ground to assume that the heating prismatic rays possess it from the circumstance of their passing through the prism, because, when the light impinges upon the prism, we know that it has not any such separate rays accompanying it; and of the nature or properties of the rays during their passage through the prism, we are altogether ignorant.

(17.) My object in making these remarks is merely to attain if possible some clear ideas on the subject in question; and to point out those parts of it which appear to me to want further elucidation; and to several of which I have attempted to direct experimental research. The subject must always remain perplexed and obscure so long as we dispute about such terms as "calorific rays," "luminous," or "non-luminous heat," &c. The only way of arriving at clearness of ideas, and thence being able to pursue the inquiry in a satisfactory manner, is to fix upon some definite criteria by which the nature of heating agents may be distinguished and compared; and instead of framing theories to explain the union of heat and light in the sun's beams, to describe the nature of the phenomena in conformity to the criteria above pointed out, as afforded by certain experimental facts.

It becomes necessary to subject to an examination by those criteria the supposed exterior heating effect of the prismatic spectrum; as well as the analogous exterior heat of the cone of light formed by a lens, and probably several other phenomena, before we can obtain from them any further information respecting the nature of the heating power which is so inseparably associated with the sun's light.

The remarks hitherto made apply to the subject of *solar* light and heat; but they might be extended also to the investigation of the relations of light and heat from terrestrial sources. The want of some fixed criteria of definition must upon consideration be felt equally in following up this part of the subject as in the former; and to endeavour to supply that want should be the first business of the experimenter.

Some further observations will probably form the subject of a future communication.

ARTICLE II.

Astronomical Observations, 1824.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

March 28.	Emersion of Jupiter's second satellite	8h	17′	24″	Mean Time at Bushey.
		8	18	45	Mean Time at Greenwich.
March 31.	Emersion of Jupiter's first satellite	8	13	49	Mean Time at Bushey.
		8	15	10	Mean Time at Greenwich.
April 5.	Occultations by the moon, Siderial Time	12	15	44	Immersion of a small star.
		12	19	01	Im. Jupiter's 4th satellite.
		12	21	19	Immersion of a small star.
		13	21	51	Em. Jupiter's 1st limb.
		13	22	01	Em. Jupiter's 2d limb.

Clouds prevented the Immersion of Jupiter being seen: at the Emersion, the limbs of Jupiter, and the Moon, were tremulous.

April 7.	Emersion of Jupiter's first satellite	10	09	15	Mean Time at Bushey.
		10	10	36	Mean Time at Greenwich.

ARTICLE III.

On the Decomposition of the Metallic Sulphates by Hydrogen Gas. By J. A. Arfwedson.

Since it has become known that the fixed alkaline sulphates may, by means of hydrogen gas, be reduced to metallic sulphurets, it was natural to infer that the same method would be equally successful with the different metallic sulphates. This consideration induced me to undertake a set of experiments by the above-mentioned method, in order to determine more accurately the nature of the combination of sulphur and manganese; concerning which chemists have been long of opinion that the manganese in it is in the state of an oxide; although several circumstances, the most important of which is its property to dissolve in acids with the evolution of sulphuretted hydrogen gas, have led also to the opposite sentiment. The experiments which I am going to relate had at first no other object than to determine the nature of this compound of sulphur and manganese; but the unexpected results obtained led afterwards to the experiments which will be related.

To avoid unnecessary prolixity in the description of the following experiments, I should state in the first place, that all the reductions of which I shall have occasion to speak, were performed in precisely the same kind of apparatus, consisting merely of a piece of barometer tube, of rather difficultly fusible glass, about the middle of which was blown a small globular cavity, into which the substance destined for reduction was put. The hydrogen gas was prepared from zinc and dilute sulphuric acid. It was dried by passing it through fused muriate of lime, before it entered the tube. In those cases where sulphuretted hydrogen gas was employed, it was freed from the accompanying moisture in exactly the same way.

Reduction of Protosulphate of Manganese by Hydrogen Gas.

In the little apparatus just described a portion of pure sulphate of manganese was put, which, though previously deprived of its water, was again heated in the apparatus till I was certain that it retained no moisture: then the evolution of hydrogen gas was set a-going, and when the whole atmospherical air had been driven out of the apparatus, the salt was heated over an Argand spirit-lamp. Till the matter became red-hot, it underwent no alteration; but at that temperature the salt began to become dark, and at the same time sulphurous acid and water came over together. When this extrication was at an end, and when the hydrogen gas passed through unaltered, the reduction was considered as completed, and the apparatus was allowed to cool, still filled with hydrogen gas. The product of this experiment was a light-green powder, which dissolved in muriatic acid, with the evolution of sulphuretted hydrogen gas; and the solution was rendered only slightly turbid by the solution of barytes. Thus it appears that the salt was completely decomposed: 1·484 gramme of sulphate of manganese thus treated lost 0·697 gr. in weight, or 46·97 per cent. In another experiment 0·553 gr. lost 0·263, or 47·56 per cent. A third experiment gave a loss of 1·034 gr. from 2·195 grs. of salt, which amounts to 47·10 per cent. The mean of these three experiments gives 47·22 for the loss of weight sustained by 100 parts of anhydrous sulphate of manganese when thus treated.

It becomes now a question of some difficulty to determine the composition of the substance thus obtained. It is impossible that it could contain the same quantity of sulphur as the salt employed ($\mathrm{Mn}\ S^2$), because a quantity of sulphurous acid had made its escape: nor could it be manganese combined with an atom of sulphur ($\mathrm{Mn}\ S$); for on such a supposition the weight lost by 100 parts of the salt should have been 52·32, which was considerably greater than that found by experiment. I thought it, therefore, likely that I had obtained a body analogous to the crocus antimonii; or that it consisted of a combination of sulphuret of manganese and protoxide of manganese. The simplest proportion in which such a combination can take place, is one atom of sulphuret with an atom of protoxide; and in order to obtain such a result from protosulphate of manganese it is obvious that 100 parts of the salt must lose 47·09; or almost the loss sustained in the preceding experiments; for $2\,\ddot{\mathrm{Mn}}\,\dddot{S}^2 : 2\,\ddot{\mathrm{Mn}}\,\dddot{S}^2 - (\ddot{\mathrm{Mn}} + \mathrm{Mn}\,S^2) :: 100 : 47{\cdot}09$. The reason of this combination may be thus explained, that the sulphate of manganese is decomposed by hydrogen gas in such a way that half of the salt is changed into $\mathrm{Mn}\ S^2$, while the other half loses its acid and remains in the state of protoxide. In order to prove the truth of this opinion, it was merely necessary to determine the quantity of sulphur in the reduced body, the quantity of manganese being already known from our knowledge of the composition of the salt; and the remainder wanting to make out the complete weight must obviously be oxygen. I attempted first to determine the sulphur by dissolving the body in aqua regia in order to oxidize the sulphur, that it might be afterwards thrown down by barytes; but the evolution of sulphuretted hydrogen gas which took place on the addition of the acid rendered this method abortive. I next dissolved the matter in muriatic acid, and made the sulphuretted hydrogen gas pass through a solution of acetate of lead; but even when this process was followed, I obtained an uncertain and varying product:

the cause of which was that the sulphuret of lead formed is partly converted into sulphate during the drying.

Still, however, the method remained of determining the quantity of sulphur by roasting the compound. A portion of the substance in question prepared just before, which I shall hereafter call *oxisulphuret of manganese*, was heated to redness in a platinum vessel. The matter took fire before it became red-hot and burnt, leaving behind brown oxide of manganese, or *oxidum manganoso-manganicum*. But in order to drive off the whole of the sulphur, a long continued roasting was necessary.

0·36 gr. of oxisulphuret gave by this treatment 0·347 gr. of brown oxide of manganese; equivalent to 96·39 per cent. The oxide of manganese thus obtained dissolved in muriatic acid without any residue, and the solution was not rendered turbid by muriate of barytes. Now if the oxisulphuret of manganese were a compound of $\ddot{M}n + Mn\,S^2$, then 100 parts of it would correspond with 96·58 parts of $\dot{M}n + \dddot{M}n^2$, or brown oxide of manganese; and the result, as is obvious, corresponds very well with this calculation.

Meanwhile, in order to be certain of the accuracy of these conclusions, I thought it necessary to make direct experiments, in order to show the existence of protoxide of manganese in body.

For this purpose a portion of protosulphate of manganese was reduced by hydrogen gas, in the way above described. After this the apparatus was weighed in order to determine the amount of the oxisulphuret remaining.* A stream of dry sulphuretted hydrogen gas was then passed through the same apparatus. It was clear that the protoxide of manganese, in case it was present, would by this process be converted into $Mn\,S^2$; consequently a certain portion of water would be formed. This accordingly took place, and with so much rapidity, that almost as soon as the gas entered the apparatus, and before any heat had been applied, the whole interior of the small glass globe in which the matter lay, was covered with small drops of water. It is possible that the reduction in this case might be accomplished without the application of any heat; but to make sure of the reduction, it is expedient to apply the heat of a spirit-lamp till the matter becomes of a low red heat; and the process must be continued as long as any moisture makes its appearance. The apparatus is then to be allowed to cool. It is to be weighed after the gas with which it is filled has been driven off, and replaced by common air. 0·933 gr. oxisulphuret treated in this way left 1·022 gr. of sulphuret. This corresponds to 100 parts of the former, and 109·54 of the latter. This is just the quantity of $\mathrm{Mn\,S^2}$ which, according to calculation, should be obtained from 100 parts of $\mathrm{\dot{Mn} + Mn\,S^2}$; for $\mathrm{\dot{Mn} + Mn\,S^2 : 2\,Mn\,S^2}$ :: 100 : 109·98.

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* The diminution of weight was quite the same as in the former experiment.

The colour of oxisulphuret of manganese is somewhat a lighter green than that of the protoxide. It remains unaltered though exposed to the air; and thus is easily distinguished from the protoxide, which, as is known, speedily absorbs oxygen, and becomes brown. It is easily distinguishable likewise from the sulphuret of manganese, which has a much darker green colour, and which moreover, when long exposed to the air, gradually becomes oxidized, and assumes a brown colour.

The constituents of 100 parts of oxisulphuret of manganese calculated from the data given above, are as follows:

Manganese	70·26
Sulphur	19·86
Oxygen	9·88
	100·00

Or,

Sulphuret of manganese	55
Protoxide of manganese	45
	100

Reduction of Protoxide of Manganese by Sulphuretted Hydrogen Gas.

It was of importance after the preceding experiments to determine whether any other sulphuret besides $\mathrm{Mn\,S^2}$ could be formed when protoxide of manganese is treated with sulphuretted hydrogen gas. I prepared, therefore, a portion of protoxide of manganese by reducing the oxide by means of hydrogen gas, and after the weight of the protoxide had been determined with the requisite precision, it was treated with sulphuretted hydrogen gas so long as any water was formed. From 0·317 gr. protoxide of manganese treated in this way, I obtained 0·392 gr. sulphuret of manganese, or from 100 parts of the former 123·66 of the latter; but $\mathrm{Mn : Mn\,S^2}$ :: 100 : 122·19. The small excess in the experimental result proceeded doubtless from this cause, that the protoxide could not be weighed with sufficient rapidity to prevent it from absorbing a little oxygen from the air.

I attempted afterwards to reduce protosulphate of manganese by means of sulphuretted hydrogen gas. 0·899 of anhydrous salt gave 0·526 of sulphuret of manganese. Now $\mathrm{Mn\,\dddot{S} : Mn\,S^2}$ :: 0·899 : 0·523. From this it seems to appear that manganese in the dry way cannot combine with more than two atoms of sulphur.

Examination of the Substance formed when Protocarbonate of Manganese is fused in a close Vessel with Sulphur.

1. Protocarbonate of manganese was intimately mixed with nearly twice its weight of washed flowers of sulphur. The mixture was put into a small retort blown at the enameller's lamp, which was afterwards slowly raised to a red heat. When no more sulphurous acid was exhaled, and when the superfluous sulphur had been volatilized and collected in the beak of the retort, the mouth of the retort was stopped with a cork, and the fire withdrawn. On cooling, the matter contained in the belly of the vessel was taken out. It had the light-green colour of oxisulphuret of manganese. It dissolved in muriatic acid with the evolution of sulphuretted hydrogen gas; but the solution was considerably affected by muriate of barytes. 0·418 gr. of it left after burning 0·392 gr. of brown oxide of manganese. Another portion, weighing 0·710 gr. was dissolved in muriatic acid, and precipitated by muriate of barytes. There were obtained 0·039 gr. of sulphate of barytes, equivalent to 0·026 protosulphate of manganese; consequently the abovementioned 0·418 gr. of sulphuretted manganese contained 0·015 protosulphate of manganese, and the remaining 0·403 had given 0·377 of brown oxide.* 0·403 $Mn + Mn S^2$ would have given 0·388 brown oxide; and the same quantity of $Mn S^2$ is proportional to 0·354 brown oxide; consequently the body under examination seems to be a mixture of $\ddot{M}n S^2$ with a smaller quantity of $\dddot{M}n$ than in the compound $\dddot{M}n + Mn S^2$.

2. It was probable that the imperfect conversion of the protocarbonate of manganese into $\dot{M}n S^3$ in this experiment, was owing to the process having been conducted too rapidly; so that the sulphur was distilled away before it had time to decompose all the $\ddot{M}n$. A new portion of protocarbonate of manganese and sulphur was, therefore, mixed together, and exposed to a heat just sufficient to keep the sulphur in the state of fusion. When in consequence of continuing this heat for several hours it was supposed that the decomposition might be completed, the heat was augmented so as to drive off the excess of sulphur, and the retort was corked and allowed to cool. In the same manner as in the former experiment, it was found that 0·922 gr. of the sulphuret of manganese now formed contained 0·036 of protosulphate of manganese. The remaining 0·886 left when burnt 0·787 of brown oxide of manganese. 0·886 $Mn S^2$ are proportional to 0·778 brown oxide of manganese. Hence it appears

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* This is agreeable to an observation already made, that no protosulphate of manganese is formed when sulphuretted manganese is burnt.

that in the latter experiment the sulphuret contained a smaller admixture of oxide than in the former.

3. A portion of the sulphuret of manganese prepared in the second experiment was accurately mixed and fused with its own weight of sulphur. 0·732 gr. of this product contained 0·031 of protosulphate of manganese, and 0·701 gr. of the remaining quantity gave when burnt 0·619 gr. of brown oxide of manganese. This quantity differs only by 0·004 gr. from the calculated quantity of oxide which should have been obtained from 0·701 $Mn\,S^2$; so that the sulphuret of manganese of this experiment appears to have been free from protoxide.

From these experiments it appears that when protocarbonate of manganese is fused with sulphur in a close vessel, there is always formed (together with a little protosulphate of manganese) a sulphuret of manganese containing more or less oxide. This is the substance which has been erroneously denominated sulphuretted oxide of manganese; and the best way to obtain a sulphuret free from oxide is to fuse this substance a second time with its own weight of sulphur.

Native Sulphuret of Manganese, or MANGANGLANSE, *from Nagyag, in Transylvania.*

In connexion with the preceding experiments, it seems of importance to determine the constitution of sulphuretted manganese prepared by nature. This mineral, according to the analysis of Klaproth, is composed of

Protoxide of manganese	82
Sulphur	11
Carbonic acid	5
	98*

Klaproth concluded that the manganese was in the state of protoxide, because he found that when protoxide of manganese and sulphur were melted together, he obtained a compound similar to the natural one as far as external characters were concerned. But the insufficiency of such a reason, connected with the circumstance that the mineral, like the artificial sulphuret of manganese, dissolves in acids with the evolution of sulphuretted hydrogen gas, seems to furnish sufficient ground for suspecting the accuracy of Klaproth's opinion.

0·494 gr. of pulverized manganglanse were heated to redness on a thin platinum plate, till they ceased to lose any more weight. Native sulphuret of manganese parts more difficultly with its sulphur than what is artificially prepared. On that account the roasting must be several times repeated, because the weight is diminished each time the process is repeated. The residual oxidum manganoso-manganicum weighed 0·425 gr. It dissolved completely in muriatic acid, and the solution was not rendered turbid by muriate of barytes, and was found to contain no foreign body, except a trace of iron. 0·494 Mn S^2 are proportional with 0·434 $\ddot{Mn}$ + 2 $\dddot{Mn}$; which does not much exceed the experimental result. We may conclude from this experiment that manganglanse is a compound of one atom manganese with two atoms sulphur. That the loss of weight was a little greater than it ought to have been was a necessary consequence of the protocarbonate of manganese mixed with the ore, which could not be completely separated, notwithstanding every possible care. Even when we find pieces which appear quite pure, if we heat them in a little glass capsule, they always become spotted with small brown flocks of decomposed carbonate. These may always be easily perceived if we examine the matter through a glass.

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* Beitrage, iii. 42.

Reduction of Sulphate of Zinc by Hydrogen Gas.

The zinc vitriol employed in these experiments was prepared by dissolving pure oxide of zinc in distilled sulphuric acid. The salt was moderately heated to render it anhydrous without driving off any of the acid. It was then treated with hydrogen gas exactly in the same way as the protosulphate of manganese. At the same temperature in which that salt was reduced, the sulphate of zinc began likewise to be decomposed; sulphurous acid and water were given out; and in a short time the reduction was completed. A little before that period, the matter swelled up and acquired a motion; at the same time its temperature augmented, in consequence of which a small portion of metallic zinc was sublimed, and attached itself to the upper part of the apparatus.

The reduced substance was pulverulent and straw-yellow. When treated with sulphuretted hydrogen gas a considerable portion of water was formed. It dissolved in muriatic acid with the evolution of sulphuretted hydrogen gas, and the solution was not rendered turbid by muriate of barytes. Hence it was evidently a mixture of sulphuret and oxide of zinc.

The loss of weight during the reduction of the salt was in three careful experiments as follows: In one experiment 0·544 gr. of zinc vitriol left 0·305 gr. or 56·07 per cent. In the next 2·933 gr. of the salt left 1·708, or 58·23 per cent; and in the third 1·166 gr. of salt left 0·664, or 56·95 per cent.

The quantity of oxisulphuret here obtained is greater than it ought to be if the compound were analogous to the oxisulphuret of manganese; that is to say, $\ddot{Zn}$ + Zn S^2; for 100 parts of sulphate of zinc correspond by calculation with 52·52 $\ddot{Zn}$ + Zn S^2, a quantity which deviates considerably from that actually obtained; and an equally great, if not a greater deviation would result if we suppose that the oxide and sulphuret are combined in other atomic proportions. It was observed that a portion of the zinc was reduced to the metallic state; but the quantity was so small that the amount of the change which it would produce cannot come into calculation. Indeed if it were to produce any change upon the result, the obvious consequence would be to render the loss of weight a little too high; whereas in all the experiments that loss was too small. From the experiment it is obvious, that the loss of weight is various in different experiments, and that it becomes less when the quantity of salt operated on is greater.

These circumstances taken together might naturally lead to the opinion that the sulphate of zinc was more or less completely decomposed in the different processes. But as has been already stated, a portion of the reduced mass was always dissolved in muriatic acid, and tested with muriate of barytes, by which the liquid was sometimes rendered slightly opalescent, but not the least precipitate ever fell. There might, however, have been some circumstance which occasioned the result to come out inaccurate, but in what it consisted I cannot say.

Meanwhile we may conclude from the preceding experiments, that when sulphate of zinc is treated with hydrogen gas, the salt is decomposed in such a way that somewhat more than the half of it becomes sulphuret, and the remainder oxide of zinc; though at the same time the proportions of each of these bodies do not correspond with any atomic numbers; and experiments which do not give corresponding results, are not worth the pains of determining the relative proportions of the constituents obtained.

Examination of Native Sulphuret of Zinc, or Zinc-Blende.

I employed for this analysis a portion of a large piece of crystallized yellow transparent blende, as the purest variety of that mineral met with in nature.

a. 1·758 gr. of pulverized blende were digested with an aqua regia, which had been previously gently warmed, so that it began to give out chlorine gas.* When the residual mass, coagulated into a lump, seemed to be no longer acted on by the acid, it was separated and washed. Dried on the stove in a platinum dish, it weighed 0·393 gr. Being heated to redness in a crucible much sulphur was driven off, but a portion remained which was sulphuret of zinc. It was dissolved in aqua regia, without any residue, and the diluted boiling-hot solution was precipitated by carbonate of potash. As soon as in consequence of the continued application of heat, all excess of carbonic acid was driven off. The precipitate was collected on the filter and washed. After being heated to redness, there remained 0·146 gr. oxide of zinc equivalent to 0·117 gr. of metallic zinc. The remainder of the 0·393 gr. or 0·276, therefore, was sulphur.

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* I have found that this is the best solvent which can be employed; for when we dissolve blende in nitric acid or in an aqua regia in which no chlorine exists, as when a dilute nitric acid is employed, there is always disengaged at first a little sulphuretted hydrogen gas which may be made evident by holding a paper dipped in solution of lead over the mouth of the vessel.

b. The sulphuric acid formed by the first digestion in aqua regia, was precipitated by muriate of barytes. There were obtained 2·288 grs. of sulphate of barytes, equivalent to 0·786 sulphuric acid, or 0·316 sulphur.

c. The residual liquid was mixed boiling-hot with carbonate of potash, taking the necessary precautions that none of it was lost in consequence of the effervescence. When no more carbonic acid was disengaged, the carbonate of zinc was collected on the filter and washed. When heated to redness, it left 1·311 gr. of zinc oxide, and, except a trace of iron, it did not appear to contain any other substance in solution: 1·311 oxide is equivalent to 1·05 metallic zinc. Thus it appears that 1·758 blende contain

Zinc *a*	0·117	
b	1·050	
		1·167
Sulphur *a*	0·276	
b	0·316	
		0·592
		1·759

Or in 100 parts:

Zinc	66·34
Sulphur	33·66
	100·00

This is two atoms of sulphur to one atom of zinc for Zn : 2 S :: 66·34 : 33·09.

Sulphate of Cobalt.

1. 0·689 gramme of pure crystallized sulphate of cobalt; but previously freed from water by heat, were treated in the way already described, with hydrogen gas. At a high temperature the salt was easily and rapidly decomposed; sulphurous acid and water were formed; and a dark grey cohering mass remained, which weighed 0·345 gr. In a subsequent experiment, the weight remaining from 0·772 gr. of the salt, was 0·412 gr. The substance thus obtained dissolved in a great measure in muriatic acid, without the extrication of any gas. When the mass was heated, it was attacked a little more by the acid, and a little sulphuretted hydrogen gas was disengaged. The undissolved

residue when heated gave out sulphur, and oxide of cobalt remained.

2. 0·379 gr. of this reduced substance, treated with sulphuretted hydrogen gas, gave out water, and the weight at the conclusion of the process was 0·440 gr. In a subsequent experiment the weight was increased from 0·224 to 0·261 gr. The number 0·440 corresponds with 116·09 to the 100 parts, and the number 0·261 with 117·04. The water obtained in this process, together with what was said above of the presence of sulphur, shows us that the sulphate of cobalt decomposed by hydrogen gas becomes likewise an oxisulphuret.

The residue per cent. when the salt is reduced, amounts in the first experiment to 53·99; and in the second to 53·24; or at a medium to 53·62: but 2 Co S^2 : Co + Co S^2 :: 100 : 53·56. From this we may conclude, that sulphate of cobalt is decomposed by hydrogen gas in the same way as protosulphate of manganese; or that one-half of the salt is converted into oxide, and the other half into sulphuret. The experiments with sulphuretted hydrogen gas ought to have corresponded with the foregoing result; but this is not the case; for the weight of Co + Co S^2 : 2 Co S^2 :: 100 : 109·7; consequently from 100 parts of the oxisulphuret I should have obtained 109·7 of sulphuret, whereas in the experiments I got as much as 117·04; but I have good reasons for presuming that this experiment formed a sulphuret containing more sulphur than Co S^2. Since the cobalt pyrites from Riddarhyttan, according to Hisinger's analysis,* consists of cobalt united to three atoms of sulphur. The number 117·04 agrees nearly with Co S^2 + Co S^3.

Sulphate of Nickel.

A portion of pure oxalate of nickel (obtained according to Laugier's method, by adding oxalic acid to a solution of nickel not free from cobalt in ammonia) was heated in a small retort till it was decomposed. The residual metallic nickel was dissolved in dilute sulphuric acid, and evaporated till it yielded crystals.

1·015 gr. of crystallized sulphate of nickel freed from moisture by exposure to heat,† were treated with hydrogen gas. This salt was decomposed as easily and speedily as the preceding salts. At first sulphurous acid and water were evolved, but at last the gas which came over had the odour of sulphuretted hydrogen. After an hour, even this odour ceased, and the apparatus was allowed to cool. The residue of this experiment weighed 0·49 gr. and was a pale-yellow cohering mass, which here and there seemed to have undergone a commencement of fusion. It was brittle, and easily reduced to a powder, which, when rubbed against a hard body, gave a whitish-yellow metallic streak; and was attracted rather strongly by the magnet. It dissolved in nitric acid, leaving a residue of sulphur. By concentrated muriatic acid, it was slowly attacked, with the evolution of sulphuretted hydrogen gas; but in dilute muriatic acid it did not seem to dissolve even when the action of the acid was assisted by heat.

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* Afhandl. i Fysik, Kemi, &c. iii. 316.

† This must be done with caution, because the salt begins to be decomposed at a cherry-red heat.

The product of this experiment could not be an oxisulphuret, because it is well known that the oxide of nickel is reduced to the metallic state by hydrogen gas. I must have obtained a sulphuret which contained less than two atoms of sulphur, as the decomposition of the sulphate of nickel was attended with the evolution of both sulphurous acid and sulphuretted hydrogen gas. It is to be presumed that the salt lost one atom of sulphur, so that the sulphuret of nickel obtained was a compound of one atom of each of its elements. Let us see how this supposition corresponds with the experiment. 1·015 gr. of sulphate of nickel left a residue of 0·49 gr. This corresponds with 48·28 from the hundred parts; but the weight of $N_1 S^2 : N_1 S :: 100 : 48{\cdot}44$. From this it is evident that the sulphuret obtained was $N_1 S$. I ought to mention likewise that the reduction of the sulphate of nickel should not take place in too high a temperature; for in that case, the sulphuret of nickel melts into lumps, in consequence of which an additional portion of sulphur is dissipated, and the loss of weight turns out too high. I obtained in an experiment made in this way from 1·095 sulphate of nickel a residue of 0·513; yet according to the preceding calculation, it ought to weigh 0.53. But I cannot with certainty affirm that this additional loss of weight is owing merely to the escape of sulphur; for at the time when the matter melts, if the gas which escapes be set on fire, it burns with a distinct green flame. From this it seems not unlikely that even some of the nickel flies off as well as of the sulphur.

I now wished to know in what respects this lower combination of sulphur with nickel differed from $N_1 S^2$. A portion of this second compound, therefore, was prepared by passing sulphuretted hydrogen over red-hot oxide of nickel. 1·186 gr. of nickel oxide gave 1·438 gr. of sulphuret of nickel, which deviates very little from the quantity $N_1 S^2$, which ought by calculation to have been obtained; for the weight of $N_1 : N_1 S^2 :: 1{\cdot}186 : 1{\cdot}441$. This combination of nickel and sulphur was pulverulent. Its colour was a somewhat darker-grey than that of the oxide. It was not in the least attracted by the magnet. It could not be fused even when exposed to a higher temperature than was necessary to fuse N S. Thus the two sulphurets differ in several respects from each other.

Analysis of native Sulphuret of Nickel, or the Mineral called Hair Pyrites.

Klaproth, who analyzed this mineral, found it to consist of metallic nickel mixed with a little cobalt and arsenic; and this conclusion was considered as correct till Prof. Berzelius, a few years ago, in consequence of a set of experiments on it by the blowpipe, concluded that hair pyrites was not metallic nickel, but sulphuret of nickel; but as no analysis of it, so far as I know, has yet been made, I conceived that the undertaking should not be neglected, especially as Prof. Berzelius had the goodness to present me for the purpose with a very beautiful piece of that rare mineral.

a. 0·222 gr. of needles of hair pyrites freed as much as possible from the small fragments of quartz with which they were mixed, were digested in aqua regia, as long as any thing was dissolved. The solution, together with a small portion of sulphur swimming upon the surface, was decanted off, the quartz grains lying at the bottom of the flask. These grains, when washed and heated to redness, weighed 0·006 gr. The undissolved sulphur was likewise separated. It weighed, after being washed and dried, 0·002 gr.

b. The solution in aqua regia was precipitated by muriate of barytes, and the sulphate of barytes obtained being collected on the filter and edulcorated, weighed, after being heated to redness, 0·524 gr. containing 0·18 sulphuric acid, or 0·072 sulphur.

c. The residual liquid was freed from barytes by sulphuric acid, and then precipitated by caustic potash. The hydrate of nickel containing potash was collected on a filter, and washed with hot water, till the liquid which passed through ceased to leave any residue when evaporated to dryness. It took several days to accomplish this. Being then dried and heated to redness, it was oxide of nickel, and weighed 0·176 gr. equivalent to 0·139 gr. of metallic nickel.

A portion of this oxide of nickel was dissolved in muriatic acid, and the solution was supersaturated with caustic ammonia. By this means the precipitate which appeared at first was dissolved, with the exception of a few inconsiderable flocks, which being examined before the blowpipe were found to be alumina mixed with oxide of iron. These flocks without doubt owe their origin to the stony matter accompanying the hair pyrites. From the ammoniacal solution the oxide of nickel was precipitated by caustic potash. The residual liquid was colourless, but when it was evaporated to dryness, and again redissolved in water, there remained a brown powder, which gave a blue glass with borax, and consequently was oxide of cobalt; but the quantity was so small that it cannot be estimated. Whether this cobalt originated from the matrix, or whether it was really a constituent of hair pyrites, cannot be determined. The second conjecture is not improbable, as all other nickel-minerals contain cobalt.

Another solution of the oxide of nickel in muriatic acid was mixed with the liquid remaining after the precipitation with caustic potash in *c*. Then a current of sulphuretted hydrogen gas was passed through this liquid after it had been mixed with a slight excess of muriatic acid. No precipitate fell, but the liquid assumed a tint of yellow indicating the presence of a trace of arsenic. If we subtract the 0·006 gr. of quartz from the 0·222 gr. of hair pyrites, subjected to analysis, there will remain 0·216 gr. which is composed of

Sulphur *a*	0·002		
b	0·072		
		0·074	or 34·26
Nickel *c*		0·139	64·35
		0·213	98·61

An atom of nickel weighs 739·51, and two atoms of sulphur 402·32; but 739·51 : 402·32 :: 64·35 : 35·02. Thus it appears that hair pyrites is a compound of one atom of nickel and two atoms of sulphur. It is not in the least affected by the magnet.

Protosulphate of Iron.

A determinate portion of pure protosulphate of iron, which had previously, by the cautious application of heat, been deprived of its water, was treated with hydrogen gas. The salt exhibited the very same appearances as sulphate of nickel under the same circumstances. Sulphurous acid and water first came over, and at last sulphuretted hydrogen gas. When the process was concluded there remained a dark-grey agglutinated pulverulent matter, which was strongly attracted by the magnet, and dissolved in muriatic acid with the evolution of sulphuretted hydrogen gas. The solution was not altered by muriate of barytes. 0·858 gr. of anhydrous sulphate of iron, gave 0·396 gr. of the new substance: 0·367 gr. of this new product were treated with sulphuretted hydrogen gas. The weight was increased to 0·474 gr.; but not the smallest portion of water was formed, showing that sulphate of iron is changed by hydrogen gas into a substance which contains no oxygen.

The experiment with the hydrogen gas was repeated on 1·012 gr. of sulphate of iron, and the residual matter amounted to 0·479 gr. The residue in the first experiment was 46·15 per cent. and in the second 47·33; the mean of which is 46·74. The weight of $Fe\,S^2$: $Fe\,S$ :: 100 : 46·82. I had thus obtained a compound of an equal number of atoms of iron and sulphur; and consequently the body contained only half as much sulphur as the compound, which we have hitherto called sulphuret of iron at minimo. The increase of weight in the experiment with sulphuretted hydrogen gas was too great to induce us to admit that the product was $Fe\,S^2$; for the weight of $Fe\,S$: $Fe\,S^2$ :: 0·367 : 0·450; whereas the experiment gave 0·474. But it is known from Prof. Stromeyer's experiments,* that what we commonly call sulphuret of iron in minimo—a substance which may be prepared artificially, and exists also as a natural production known under the name of magnetic pyrites, is not $Fe\,S^2$; but a compound which may be represented by the formula $Fe\,S^4 + 6\,Fe\,S^2$, so that it contains more sulphur than $Fe\,S^2$. The constituents per cent. of magnetic pyrites are:

Iron	59·85
Sulphur	40·15
	100·00

In my experiment, 0·367 Fe S were operated upon, which contain 0·283 iron; the sulphuretted hydrogen gas augmented the weight to 0·474. This new compound of course contains 0·283 iron and 0·191 sulphur, or in 100 parts, 59·7 iron and 40·3 sulphur. Thus the sulphuretted hydrogen had furnished the quantity of sulphur requisite to convert Fe S into magnetic pyrites.

Subsulphate of Iron.

This salt is obtained, as is known, when a smaller quantity of caustic potash is added to a solution of persulphate of iron than is requisite to separate the whole of the iron. As it contains only one atom acid united to two atoms of the basis, I expected that the hydrogen gas would have converted it into $Fe^2\,S$. But contrary to expectation, sulphurous acid and sulphuretted hydrogen were disengaged during its reduction 0·709 gr. prepared in the above described way, and free from water, left for residue 0·422 gr. But a long time elapsed before the sulphuretted hydrogen gas ceased to come over, although the mass was kept in a full red heat. The matter was in appearance similar to metallic iron, such as it is formed when the oxide is reduced by hydrogen gas. It was acted on almost as strongly by the magnet as that, and was semimalleable, but it dissolved in muriatic acid with the evolution of sulphuretted hydrogen. The quantity of the reduced body, compared with that of the salt employed, shows that the product was a compound of four atoms iron with an atom of sulphur, or that the salt lost half of its sulphur, togethat with all its oxygen; for the weight of $2\,\dddot{\text{Fe}}\,{}^2\dddot{\text{S}} : \text{Fe}\,{}^2\text{S} :: 0{\cdot}709 : 0{\cdot}420$. The residue in the experiment was only a very little higher, or 0·422.

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* Gilbert's Ann. xviii. 186.

Thus to the sulphurets of iron already known, viz. $\text{Fe}\,\text{S}^4$ and $\text{Fe}\,\text{S}^2$, we can now add $\text{Fe}\,\text{S}$ and $\text{Fe}^4\,\text{S}$; and probably that series will be still further increased with $\text{Fe}\,{}^2\text{S}$; so that it will be possible to exhibit a sulphate of iron, so constituted that the number of atoms of the acid shall be equal to those of the base.

Sulphate of Lead.

The salt was decomposed by hydrogen gas with facility, and sulphurous acid was disengaged, followed at last by sulphuretted hydrogen. The product obtained was a mixture of sulphuret and metallic lead, which, towards the end of the process, had melted together in small balls, which were quite malleable. On the solution of the mass in nitric acid, a considerable portion of sulphur was disengaged. 1·294 gr. of sulphate of lead previously exposed to a red heat, left for a residue 0·940 gr. Hence it appears that a little more than half of the salt is converted into metal, and the remainder into sulphuret of lead; for the weight of $2\,\dot{\text{Pb}}\,\dddot{\text{S}}^2 : \text{Pb} + \text{Pb}\,\text{S}^2 :: 1{\cdot}294 : 0{\cdot}952$.

Whether in a higher temperature hydrogen gas cannot change the whole sulphate of lead into metal, was not tried; but it is less probable, as we see by Berthier's experiments,* that if sulphate of lead be mixed with charcoal powder, and heated in a hessian crucible even to whiteness, there always remains a mixture of metallic and sulphuretted lead.

The preceding experiments were tried also with sulphates of copper, bismuth, tin, and antimony; but none of these trials gave any very remarkable results. The copper and bismuth salts were reduced to pure metals. The tin salt gave metallic tin still mixed with some sulphuret, and from the antimonial salt was obtained a residue of oxidized metallic and sulphuretted antimony.

ARTICLE IV.

An Analysis of some Minerals. By Aug. Arfwedson.†

Cinnamon Stone from Malsjö.

DURING a mineralogical journey in Vermland during the summer of 1820, Prof. Berzelius met with a garnet mineral in the lime quarry of Malsjö, in the neighbourhood of Philipstad, which in its external characters bore a strong resemblance to the cinnamon stone of Ceylon; and he afterwards, by a comparative set of experiments before the blowpipe, satisfied himself that the two minerals possessed very similar characters. I hope by the following analytical experiments to be able to show that this mineral comes very near cinnamon stone even in its chemical composition.

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* Ann. de Chim. July, 1822, p. 276.

† Translated from the Kongl. Vetenskaps Academiens Handlingar, for 1822, p. 87.

By concentrated muriatic acid, the stone is not in the least acted upon, at least not while cold, excepting that the few attached fragments of calcareous matter are gradually dissolved.

1·526 gramme of the mineral purified in this way, and afterwards reduced to a fine powder, were heated with three times its weight of carbonate of potash. The fused greyish mass was dissolved in muriatic acid. There remained a quantity of silica, which, after being heated to redness, weighed 0·625 gr. (*a*).

The muriatic solution was precipitated with caustic ammonia with the usual precautions to prevent the alumina from being again dissolved; and the precipitate which evidently contained iron was collected on a filter, and washed with hot water. It was again dissolved in muriatic acid, and supersaturated with caustic potash; by which the precipitate which fell at first was again redissolved, with the exception of a little oxide of iron, which, after exposure to a red heat, weighed 0·067 gr. Being again dissolved in muriatic acid, it was found to contain 0·007 gr. of silica (*b*); consequently the weight of the oxide of iron was 0·06 gr. (*c*).

From the alkaline solution, the alumina was separated by muriatic acid and carbonate of ammonia. Its weight, after exposure to a red heat, was 0·321; but when dissolved in sulphuric acid, it left 0·007 gr. of silica (*d*); so that the true weight of the alumina is 0·314 (*e*).

From the liquid which had been treated with caustic ammonia, and which had been again rendered neutral by a few drops of muriatic acid, the lime was precipitated by oxalate of ammonia. The oxalate of lime obtained was well washed with warm water, dried, heated to redness mixed with a little liquid carbonate of ammonia, and heated gently till all the ammonia was disengaged. The carbonate of lime obtained in this way amounted to 0·920 gr. equivalent to 0·518 gr. of pure lime (*f*).

The liquid thus freed from lime was mixed with a sufficient quantity of carbonate of potash, and evaporated to dryness. The dry residue when dissolved in water left a substance, which, after being exposed to a red heat, weighed 0·006 gr. and which possessed the characters of oxide of manganese mixed with a little magnesia (*g*).

A determinate quantity of the mineral in coarse powder was exposed to a red heat in a platinum crucible; but lost no weight.

Thus the constituents of the mineral have been found to be:

		Per cent.		
Silica (a) 0·625 (b) 0·007 (d) 0·007	0·639	or 41·87	containing oxygen	= 21·06
Alumina	0·314	20·57		= 9·60
Lime	0·518	33·94		= 9·53
Oxide of iron	0·060	3·93		= 1·20
Manganese with magnesia.......	0·006	0·39		
	1·537	100·70		

We perceive from this table that the oxygen of the silica is equal to that in the bases. Further, that the alumina and lime have the same quantity of oxygen, and each eight times as much as the oxide of iron. Thus the formula exhibiting the constitution of the mineral is $FS + 8AS + 8CS$.

Klaproth's analysis of cinnamon stone from Ceylon gave*

Silica	38·80
Alumina.........................	21·20
Lime	31·25
Oxide of iron....................	6·50
Loss	2·25
	100·00

That result does not deviate far from mine; but as far as regards the smallest ingredient, oxide of iron, the considerable excess of it occasions quite a different formula. It becomes $FS + 4CS + 5AS$. It is possible that Klaproth underrates both the lime and the alumina, because he separated the former by carbonate of soda, and the latter from its solution in potash by means of sal ammoniac. His formula, therefore, may be a good deal faulty. The formula which I found is considerably simpler, and, therefore, more likely to be correct.

I conceive that we have thus found reason to consider the mineral from Malsjö just analyzed as a real cinnamon stone, at least as long as Klaproth's analysis continues unrepeated and unconfirmed.

Brasilian Chrysoberyl.

Our knowledge of the constituents of this mineral is derived from Klaproth's analysis,† according to which it is composed of

Alumina.........................	71·50
Lime	6·00
Oxide of iron....................	1·50
Silica..........................	18·00
	97·00

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* Beiträge, v. 142.

† Ibid. i. 102.

In a set of experiments which Prof. Berzelius made upon almost all minerals by means of the blowpipe, he expressed his opinion respecting this mineral that it did not contain lime as an essential constituent, but that in all probability chrysoberyl is a pure subsilicate of alumina.

I have found this opinion confirmed by the following analytical experiments. On that account they may deserve to be stated in this place.

Analysis.

0·614 gramme of the mineral were reduced to a fine powder in the agate mortar, and afterwards separated from all the coarser particles by washing. This powder was mixed with a sufficient quantity of caustic potash, and raised to a red heat in a silver crucible. After the heat had been kept up a full hour, the mass was found in a state of semifusion. It was washed out of the vessel with water, and treated in the usual way with muriatic acid, which left undissolved 0·238 gr. This residue was heated again with potash, and dissolved in muriatic acid. The undissolved portion now weighed 0·137 gr. The heating with potash was repeated still another time, and there now remained undissolved by the muriatic acid 0·108 gr. which, on examination, proved to be pure silica.* (*a*)

The solutions in muriatic acid were mixed with the water employed to edulcorate the silica, and the liquid was precipitated by caustic ammonia added in as small excess as possible. The precipitate, after being well washed, and heated to redness, weighed 0·507 gr. When dissolved in sulphuric acid, it left 0·007 gr. (*b*) of silica, and the solution gave with caustic potash a precipitate, which was again redissolved by an additional dose of the potash, with the exception of some flocks of peroxide of iron which could not be weighed. The matter dissolved by the sulphuric acid was of course alumina, and its quantity (subtracting the silica) was 0·500 (*c*).

For the greater security, the alkaline solution was saturated with muriatic acid, till the precipitate was redissolved, after which carbonate of ammonia was added in great excess. Had any glucina or yttria existed in the matter, it would have been dissolved by this excess of carbonate of ammonia, and would have fallen when the filtered liquid was boiled till the excess of ammonia was driven off; but the liquid stood this test without any precipitate appearing.

The liquid which had been precipitated by caustic ammonia was neutralized by muriatic acid, and mixed with some drops of oxalate of ammonia; but even after the interval of 12 hours, the liquid had not the least appearance of turbidness; nor could any precipitate be produced by boiling it after adding a little carbonate of potash.

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* To satisfy myself whether the silica be pure, I am in the habit of fusing it with a good quantity of carbonate of potash. If the fused mass dissolve in water without residue, I consider the silica as free from any admixture of foreign earth.

0·614 gr. of this mineral, therefore, contain

Silica (*a*)	0·108		
(*b*)	0·007		
		0·115 or	18·73
Alumina (*c*)		0·500	81·43
		0·615	100·16

18·73 silica contain 9·42 oxygen, and 81·43 alumina contain 38·03 oxygen; but 9·42 × 4 = 37·68. According to these proportions, the formula for chrysoberyl is $A^4 S$.

Boracite from Lüneburg.

Prof. Stromeyer states in a letter, of which an extract is inserted in Gilbert's Annalen, xviii. 215, that he analyzed this mineral, and found it a compound of 67 boracic acid and 33 magnesia. As Stromeyer's analyses have in general very justly acquired the confidence of the public, we ought not to call their accuracy in question upon slight grounds; but as we are still ignorant of the method of analysis which he employed, it is the more difficult to give implicit confidence to his statement, that all the methods hitherto known for separating boracic acid from its combination, only accomplish their object in a very imperfect manner.

In some of my experiments to extract boracic acid from its compounds, I found that if a borate (for example, *borax*) be mixed with three or four times its weight of finely pulverized and pure fluor spar, together with a sufficient portion of concentrated sulphuric acid, and the matter be afterwards heated to dryness, and then exposed to a red heat, we can in that way separate the whole of the boracic acid in the form of fluoboric acid gas. The quantity of the basis being then determined, which it may be with precision, we obtain the true composition of the salt subjected to analysis.* This analytical method may be applied to all borates with incombustible bases, as far as they are decomposable by sulphuric acid; and boracite being in this predicament, I was in hopes that I had it in my power to repeat Stromeyer's analysis with the prospect of obtaining a correct result.

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* In two experiments made in this way, I have found anhydrous borax composed of

	First Exp.	Sec. Exp.
Boracic acid	68·6	69·2
Soda	31·4	30·8
	100·00	100·00

In order to free the boracite as completely as possible from all admixture of the matrix, consisting of gypsum, a portion of it reduced to a fine powder was boiled repeatedly with water. It was afterwards collected on the filter, washed, and dried.

0·849 gramme of it were mixed in a platinum crucible with 3 grammes of Derbyshire fluor spar in fine powder, made into a paste with concentrated sulphuric acid, and then heated till it was reduced to dryness, taking the requisite precautions to avoid the dissipation of any portion from the effervescence, and the evolution of the gas which took place. The dry mass was then exposed to a red heat. For the greater security, the process was repeated, but no fluoboric gas was disengaged; showing that the mineral had been completely decomposed by the first process.

The sulphate of magnesia was then extracted by water, and the undissolved portion was washed so long upon the filter that I was sure none of the magnesia remained mixed with the gypsum. The filtered water was then freed from the gypsum which it contained in solution by oxalate of ammonia. It was then evaporated to dryness, and exposed to a red heat. The salt thus obtained weighed 0·758 gr. and possessed the characters of pure sulphate of magnesia. The quantity of magnesia in it amounted to 0·257, and consequently the remaining 0·592 gr. necessary to make up the weight of the boracite must have been boracic acid.

100 parts of boracite then contain

Boracic acid	69·7
Magnesia	30·3
	100·0

Gay-Lussac and Thenard found that boracic acid contains 33 per cent of oxygen. If this be the case, the oxygen in 69·7 is 23. 30·3 parts of magnesia contain, on the other hand, 11·73 parts of oxygen; but 11·73 × 2 = 23·26; so that boracic acid contains twice as much oxygen as magnesia. As long as the constitution of boracic acid remains disputed, I will not allege this coincidence as a proof of the accuracy of my analysis; but merely as a circumstance from which Gay-Lussac and Thenard's conclusions may receive some support.

ARTICLE V.

Addition to Mr. Herapath's Theory of Evaporation in the Annals for November, 1821. By John Herapath, Esq.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Cranford, April* 12, 1824.

IN Prop. 14 of my theory of evappration, it is said that the absolute evaporation is not affected by the pressure of any superincumbent air. This is to be understood of the decomposition at the surface of the evaporating body, and not of that escape or dispersion of vapour which constitutes apparent evaporation. Against the indiscriminate confusion of these things hints are given, I believe, in more than one place in my theory; but my attention having been recalled to the subject by the kind inquiries of the Rev. E. W. M. Rice, some views respecting the effects of pressure by airs have occurred which I intend here to notice in order to prevent misconception on certain points of my former theory.

If an evaporating surface were placed in an indefinite vacuum, it is plain that little, or perhaps none, of the emitted vapour would be recondensed on the body, but would expand into space. But were any body as an air, for instance, to be confined over the evaporating body, the particles of this air would of course come in contact with many particles of the ascending vapour, and striking them in all directions some would necessarily be beaten back on the evaporating body. Of these a considerable portion would most probably be recondensed. But whatever be the number recondensed, they will evidently be as the number beaten back to the surface, and they as the number struck by the particles of the air. Again the number struck in a given time must be as the number of particles of the air which in the same given time would pass through or strike a given space. Now this number multiplied by the momentum of a single particle, that is by the temperature of the air, is equal to the elastic force of the medium. The number of recondensed particles, therefore, or the momentary effect of a superincumbent air on the incremental evaporation is as the elasticity of the air directly, and its temperature inversely. And as this is demonstrated of no particular air, it is true of any air.

Hence, therefore, *if two evaporating bodies of the same kind be placed in any two airiform unattracted media, respectively of the temperatures of their contained evaporating bodies, the momentary effects of the media on the evaporations will be in a ratio compounded of the elasticities directly, the incremental evaporations directly, and the temperatures inversely.*

And if the temperatures are equal, the momentary effects are *directly as the elasticities and rates of evaporation; and when the elasticities are equal, these effects are inversely as the temperatures, and directly as the rates of evaporation. Of course in the same fluid at the same temperature, the incremental diminution is as the pressure directly.*

It would be easy to reduce the above laws to a comprehensive integral expression; but as I know not a single experiment with which it can be compared, I am not disposed to indulge in useless calculations. Besides it appears to me there is another cause very materially influencing the quantity of apparent evaporation; and of which I am likewise destitute of any experiments to afford me the least aid towards numerical comparison. It is this. In the above views we have regarded the superincumbent air as totally devoid of gravitation, and affecting the evaporation by its elastic force only. This however seems not to be the whole operating cause. Though by our theory, confirmed by experience, all airs in contact pretty rapidly intermix, and the rising vapour is therefore incessantly intermixing with the surrounding air, yet when the temperature is high, the vapour must evidently be liberated faster than the dispersion by intermixture can proceed; and consequently the relative specific gravities of the vapour and surrounding air tend much to suppress or elevate the vapour. A heavier air would accelerate the ascent of the vapour, and by this means contribute to diminish the recondensation; and hence increase the apparent evaporation. But an air as light or lighter than the vapour would not favour the vapour's ascent, and would, therefore, diminish the apparent evaporation by increasing the recondensation. Consequently *if the evaporating fluid, its temperature, and the pressure of the superincumbent air be the same, the greater the specific gravity of the surrounding air, provided it exceed that of the vapour, the greater the apparent evaporation.*

I am not aware that this phænomenon has ever been noticed; but if philosophers who may be engaged on this part of physics would have the goodness to attend to these hints, they may probably unfold an interesting set of laws, and add something perhaps important to our present stock of knowledge.

Several ways have occurred to me of evolving the temperature in a function of the quantity of water left, supposing evaporation alone to affect the temperature, and that at a given temperature there was a given quantity of water; but the utter defect of experiments again prevents my proceeding. However, the following views of the subject rest on principles so simple and susceptible of modification, if experiments should require it, that I am induced to give them a place here, if it be only to excite philosophers to present us with some experiments on this interesting part of evaporation. Though in my theory of evaporation I have said that the vapour arises from a decomposition at the surface, it is evident that no theoretical views only can furnish ns with the fact, whether this decomposition takes place at the mathematical surface, or at a very small distance beneath; nor can we from theory alone decide whether the vapour and its fluid have a common temperature, or even nearly so, before the former quits the latter. The well-known fact that the vapour freely ascending from boiling water is, near the surface of the water, not at the very reduced temperature to which the decomposition alone would have brought it, but at the temperature of the water, tends certainly to favour this idea. Hence if it be assumed that every portion of vapour when it quits the water has the same temperature as the remaining water; and if w be the quantity of water; t its temperature, and dw the momentary evaporation, we shall have by Cor. 1, Prop. 20, *Annals* for Dec. 1821, $\frac{11\,dw}{6}$ for the number of vaporous particles in dw. Therefore,

$$\frac{tw}{\frac{11}{6}dw + w - dw} = \frac{6tw}{6w + 5dw}$$

is the common temperature to which the water and vapour would be reduced by the decomposition of dw quantity of water. Consequently,

$$dt = t - \frac{6tw}{6w + 5dw} = \frac{5t\,dw}{6w + 5dw}$$

$$\text{or, } \frac{dt}{t} = \frac{5}{6} \cdot \frac{dw}{w}$$

whose integral is $t = A\,w^{\frac{5}{6}}$,

A being an arbitrary constant equal to

$$\frac{t_1}{w_1^{\frac{5}{6}}}$$

where t_1 and w_1 are known corresponding, or the primitive values of t and w.

If, therefore, the transmission of temperature by water was instantaneous, and every portion of vapour had its temperature equalized, as soon as formed, with that of the remaining water, and then quitted the water entirely, the temperature would be as the quantity of water left raised to the $\frac{5}{6}$th power; provided evaporation only interfered. But we know that water is not what is called a perfect conductor of heat, and therefore that the above law is not mathematically true. As the evaporation, however, is generally but trifling, unless in high temperatures, this theorem may probably be found a tolerable approximation.

Were the number of the particles increased in the ratio of 1 to r by the decomposition, we should have

$$t = A\,w^{r-1}.$$

If, therefore, $r = 100$, $t \propto w^{99}$. Hence if w be diminished by a hundredth part only, the values of t corresponding to it

100 and $u = 99$ will have a ratio equal to that of 1 to ·36973, or 100 to 37 nearly. And therefore if the true temperature corresponding to $u = 100$ be 1172·6 or 212° Fahr. the true temperature corresponding to $u = 99$ will be only 433·5, or about − 361° Fahr.; so that here for a loss of weight of only the $\frac{1}{100}$th part, the temperature would be sunk nearly 600° of Fahr.

We see from the preceding calculation that where the number of particles is much increased by the decomposition, a very trifling or even an utterly inappreciable loss of weight may nevertheless be accompanied by a very considerable diminution of temperature. Hence may easily follow the phænomena of radiation, and even those of the emission of light. Indeed I am fully convinced from many circumstances that evaporation, radiation, and the emission of light, are phænomena similar in kind, and governed by similar, but not precisely, the same laws. Probably the chief difference lies in the inequality of magnitude in the particles when decomposed. Light is most likely the particles decomposed into their smallest parts, which, if they could be inclosed within a vessel whose pores would not allow them a passage, would form a body similar to gas or vapour of extreme levity; and, on the contrary, if vapour was formed in vacuo, its particles would fly off in direct lines, like light with a great velocity. These views are rendered interesting not merely from their simplicity and their reducing to one simple cause phænomena apparently so very different as vapour and light, but they take from the sun that exorbitant temperature which certain philosophers have hitherto supposed it to have, and thus render it much more fit for a habitable globe. For since the molecules of light are conceived to be many times less than the particles of common matter, they would have with only the same momentum or temperature a velocity many times greater than the latter; and, therefore, after a particle of common matter had been struck by a molecule of light, and was returning to the other particles of the body of which it formed a part, it may be overtaken and struck again and again before it reached them by other molecules, each impulse of which would add to its motion, and therefore to the temperature of the body; and if the rays of light be condensed by a mirror or lens on any particular body, the probability of this augmented temperature would be in proportion to the density of the light. Hence, therefore, the sun may in reality have a temperature not higher than that of any other body of the system, and yet produce all the effects attributed to it. This idea comes up nearly to that broached by the late Sir W. Herschel, but without the aid of his luminous atmosphere. I need, I hope, scarcely observe, that these reiterated impulses by different molecules of light on the same particle of matter, do evidently not at all affect the theory of gases I have heretofore laid down.

It was my intention here to extend the theorems I have published in my theory of evaporation, and to add some new ones since discovered respecting the "conducting power" of gases, the agreement of which with the experimental laws of Dulong and Petit, contributes another strong link to the chain of evidence adduced in favour of my views; but as I may at some period print something in a separate form on the important subjects I have discussed in various parts of the *Annals*, I intend to reserve these, and other things which I have been able to effect in different parts of the physical sciences, to that opportunity.

ARTICLE VI.

Chemical Examination of Analcime, Copper Pyrites, and Sulphuret of Bismuth. By M. Rose.*

The first research into the composition of analcime was made by M. Vauquelin. He found this mineral to contain

Silica	58·0
Alumina	18·0
Water	8·5
Soda	10·0
Lime	2·0
Iron	Trace
	96·5

The analcime which I analyzed is found in the valley of Fassa, in the Tyrol; the crystals were trapezoidal, and were in some parts of a slight red colour; but the analysis indicated scarcely perceptible traces of oxide of iron: the crystals were translucent; and free from other impurities. The analcime reduced to powder, lost, by being strongly heated, 8·27 per cent. of water, which was slightly alkaline. By heating some fragments to redness, the loss in different experiments amounted to 8·80, 8·86, and 8·96 per cent. The analcime lost its transparency, and became a white enamel.

The analysis of analcime is not complicated, since it is very easily decomposed by acids, when it has not been heated, and is reduced to powder. On this account it is not requisite to employ nitrate of barytes used by M. Vauquelin, and which always occasions more or less loss. When digested in muriatic acid, it formed a jelly, which was dried, and afterwards treated with muriatic acid diluted with water to separate the silica. The filtered solution contained neither lime nor magnesia, and was decomposed by carbonate of ammonia; the precipitate, except a slight trace of silica and oxide of iron, was dissolved by potash. The solution, after the silica and alumina were separated, was evaporated to dryness, and the residuum was heated until all the muriate of ammonia was volatilized. The residuum gave cubic crystals of common salt free from potash. The results of the analysis were

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* Extracted from the Annales de Chimie et de Physique, tom. xxv. p. 192.

Silica	55·12
Alumina	22·99
Soda	13·53
Water	8·27
	99·91

"I found upon trial that the white transparent analcime, which is found in the lava of Catana, in Sicily, was of similar composition; but the quantity I possessed was too small to make a complete analysis, and the fragments were mixed with carbonate of lime.

I analyzed another analcime from Fassa, different from that which I have just mentioned, and I found that its composition agreed perfectly with that of the preceding; the results of the analysis were

Silica	56·47
Alumina	21·98
Soda	13·78
Water	8·81
	101·04

This analcime was of a flesh-red colour, on which account the name of *sarcolite* has been given to the analcime of Montecchio Maggiore, near Vicenza, and in which M. Vauquelin found

Silica	50·0
Alumina	20·0
Water	21·0
Soda and potash	4·5
Lime	4·5
Iron	Trace
	100·0

I cannot explain the great difference between M. Vauquelin's analysis and mine; I think I have, however, some reason for supposing that the sarcolite which he analyzed was not quite pure; for sometimes the large crystals of analcime which are found in the valley of Fassa, are so filled with perfectly well defined crystals of apophyllite, that they form almost half the mass. In the crystal of analcime which I analyzed, I found a crystal of apophyllite of an inch long. It is sometimes difficult to separate the mass of the analcime from these crystals of apophyllite; the latter, however, are discovered by their pearly lustre, and especially by their distinct cleavage, which is parallel to the plane perpendicular to the axis. It follows nevertheless from my analysis, in which I could discover neither lime nor potash which occur in apophyllite, that the fragments which I employed were perfectly free from this substance.

Copper Pyrites.

There are many analyses of copper pyrites; but they do not present any probable formula, and differ from each other. I have analyzed three sorts of crystallized copper pyrites, and I found in all of them the same proportions of constituent parts.

The analysis of copper pyrites, although very simple, never gave me results which corresponded with a probable formula, when I separated the oxide of iron from the oxide of copper by pure ammonia. The oxide of iron precipitated always contained, even after having been perfectly washed, a considerable quantity of oxide of copper, which I could not separate if I did not precipitate the copper from the muriatic solution of oxide of iron by means of sulphuretted hydrogen.

I dissolved in each analysis of copper pyrites, two different quantities in *aqua regia.* I precipitated one of the solutions by muriate of barytes to determine the quantity of sulphur by the sulphate of barytes obtained, and I poured into the other pure ammonia in excess. The oxide of iron precipitated was heated to redness, and dissolved in muriatic acid, which always left a small quantity of silica. I then treated, as already mentioned, the solution with sulphuretted hydrogen.

I obtained the following results from two varieties of crystallized copper pyrites from Ramberg, in Sayn, and from Furstenberg:

	Ramberg.	Furstenberg.
Copper	34·40	33·12
Iron	30·47	30·00
Sulphur	35·87	36·52
Silica	0·27	0·39
	101·01	100·03

The analysis of the crystallized copper pyrites from Freiberg, gave no similar results.

Sulphuret of Bismuth.

This sulphuret from Riddarhyttan, in Sweden, I found to possess the same composition as the artificial sulphuret of bismuth; namely,

Sulphur	18·72
Bismuth	80·98
	99·70

The artificial compound gave

Sulphur	18·49
Bismuth	81·51
	100·00

Note.—In this abridgment of M. Rose's paper, I have not given all his views of the atomic constitution of copper pyrites; for as he adopts the numbers of Berzelius in which the weight of the atom of iron is double that generally admitted in this country, the statements would be useless. He considers its probable composition as one atom of sulphuret of copper with three atoms of sulphuret of iron. It will, however, appear, that his analysis of copper pyrites from Ramberg comes more nearly than mine to what I have supposed to be the atomic constitution of this compound; namely, a compound of two atoms of protosulphuret of iron and one atom of persulphuret of copper, or in 100 parts of

Copper	34·78
Iron	30·44
Sulphur	34·78
	100·00

(See *Annals*, N. S. vol. iii. p. 301.)—*Edit.*

Article VII.

A Memoir on some Geometrical Principles connected with the Trisection of an Arc. By John Walker, Esq. formerly Fellow of Trinity College, Dublin. (With a Plate.)

(To the Editor of the *Annals of Philosophy.*)

SIR, *April* 14, 1824.

The following propositions I believe are new; and, if I mistake not, they either extend, or promise to extend, the limits of Plane Geometry.

It has long seemed to me, that the moderns have too much abandoned the attempt of trisecting an arc, by the right line and circle. We know with what ardour the solution of that problem was sought by the ancient mathematicians; as well as how considerably the science was advanced by their investigations,

Fig. 1.

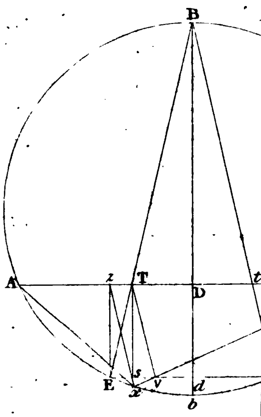


Fig. 2.

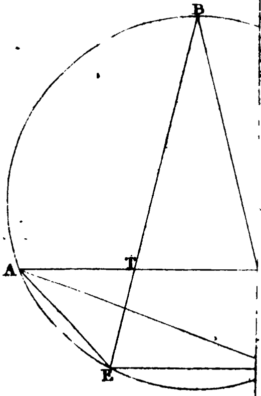


Fig. 3.

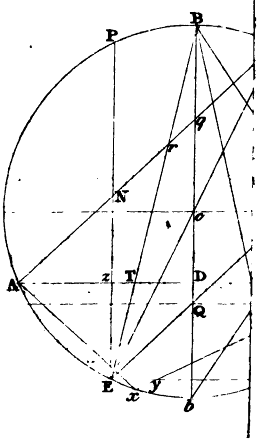


Fig. 10.

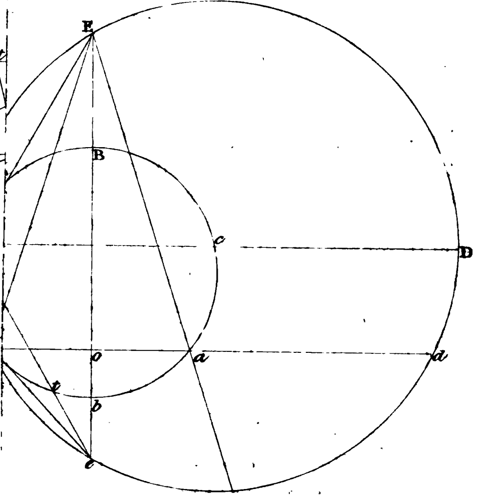
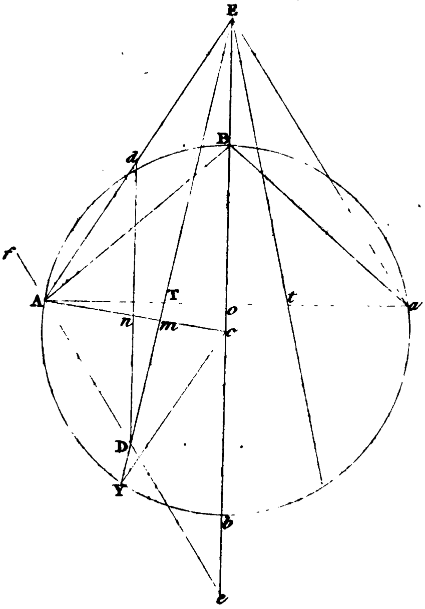


Fig. 5.



though they failed of arriving at the ultimate object which they pursued. And I believe that geometrical science would not have remained as stationary, as it has been for many years, if the problem had continued to share the attention of modern geometers.

But nothing so much paralyzes exertion as despondency of success; and I am aware that some of the most profound mathematicians of the present day do not hesitate to pronounce, that the problem alluded to is *impossible*. Their assertion is, I think, unwarrantable and rash. If I may hazard an opinion, in which many will think me over sanguine, I avow my expectation that some of the principles, which I offer in this Memoir, will yet lead to the solution of that problem; though I am now obliged to dismiss the subject from my mind, on account of other avocations and declining health.

PROP. I. PROBLEM. (Plate XXVII.) Fig. 1.

In a given segment of a circle to inscribe a triangle, whose sides shall be in arithmetical progression.

Let the given segment be $A\,b\,a$. From B, the vertex of the opposite segment, draw the lines $B\,T$, $B\,t$, trisecting $A\,a$ in T and t, and meeting the periphery in E and e. From A inscribe the chord $A\,x$ equal to $E\,e$; and draw $a\,x$. I say that $A\,x\,a$ is the triangle required.

Dem.—Let y be the point in which $a\,x$ intersects $E\,e$. Draw $T\,y$: and parallel to this draw $x\,z$, meeting $A\,a$ in z, and intersecting $E\,e$ in s. Lastly, draw $E\,z$ and $s\,T$. I shall now prove that $a\,x$ is equal to $a\,z$, and that $E\,z$ is perpendicular to $A\,a$; whence it will follow that $a\,z$, or $a\,x = \frac{A\,a}{2} + \frac{E\,e}{2}$, and is therefore an arithmetical mean between $A\,a$ and $E\,e$, i. e. between $A\,a$ and $A\,x$.

For the triangles $B\,T\,t$, $a\,t\,F$ are similar [having the angles at t equal, and also the angles at B and a, as standing upon equal arcs]. $\therefore B\,t : T\,t :: (a\,t)\,T\,t : t\,F$. $\therefore$ rectangle $B\,t\,F = T\,t^2$; and $2\,B\,t\,F = 2\,T\,t^2 =$ rectangle $A\,t\,a = B\,t\,e$. $\therefore t\,e$ is bisected in F. $\therefore$ the similar triangles $a\,t\,F$, $y\,e\,F$ are mutually equilateral, and $y\,e = a\,t = T\,t$. $\therefore e\,t$ is parallel to $y\,T$, and $\therefore$ to $x\,z$. $\therefore a\,x = a\,z$. But also $E\,y$ is bisected in s [for the triangles* $E\,y\,x$, $a\,y\,e$, and $x\,y\,s$, $F\,y\,e$, being similar, $E\,y : (y\,x)\,y\,s :: a\,y : (y\,e)\,y\,F :: 2 . 1$]. $\therefore E\,s = z\,T$, and $E\,z$ is parallel to $s\,T$, and perpendicular to $E\,e$. $\therefore a\,z$ (or $a\,D + D\,z$) i. e. $a\,x = \frac{A\,a}{2} + \frac{E\,e}{2} = \frac{A\,a + A\,x}{2}$. $\therefore a\,x$ is an arithmetical mean between $A\,a$ and $A\,x$. Q. E. D.

Cor. 1. (Fig. 2.)—The lines $A\,b$, $a\,b$ trisect the line $E\,e$; for since the line $t\,e$ bisects the angle $A\,e\,a$, $A\,e : (a\,e)\,A\,E :: A\,t :$

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* The lines $E\,x$, $e\,a$, are not drawn in the figure, but may be easily supposed.

$a\,t :: 2 : 1$. ∴ $A\,h$, bisecting the angle $E\,A\,e$, cuts $E\,e$ into segments in the same ratio.

Cor. 2. (Fig. 3.)—Drawing the diameter $E\,p$, and the line $A\,p$, the latter bisects the radius $B\,c$ in q, and trisects $B\,E$ in r. Let N be the point in which $E\,P$, perpendicular to $E\,e$, intersects $A\,p$. The triangles $E\,N\,p$, $e\,y\,a$ are similar [for the angles at p and a stand on equal arcs, as also the angles $N\,E\,p$ and $A\,a\,x$; but $A\,a\,x = a\,y\,e$]. ∴ $EN : E\,p :: y\,e : y\,a :: 1 : 2$. ∵ EN = radius. ∴ $c\,q$ = semiradius. But the triangles $E\,N\,r$, $B\,q\,r$ are similar. ∴ $E\,r : r\,B :: EN : q\,B :: 2 : 1$. ∴ $E\,B$ is trisected in r. It is plain, that if from E we draw $E\,g$ parallel to $A\,p$, it must bisect the radius $b\,c$ in Q.

Cor. 3. (Fig. 3.)—Drawing through Q an ordinate (which is the base of an inscribed equilateral triangle), the lines $B\,e$, $b\,g$ intersect this ordinate in the same point o. For the arcs $A\,E$, $p\,g$ being equal, as also the arcs $E\,b$, $p\,B$, ∴ the arc $A\,b = B\,g$. ∴ $b\,g$ is parallel to $A\,B$. ∴ the triangle $B\,h\,b$ is isosceles. But its altitude $h\,o$ is trisected by $B\,e$ [as $B\,e$ trisects $a\,D$]. ∴ the side $b\,h$ is bisected by $B\,e$ in o. [For universally, if from an angle at the base of an isosceles triangle a line be drawn cutting the altitude and leg, the segments of the leg are to each other as the upper segment of the altitude to twice its lower segment.] But the ordinate through Q, the middle point of $b\,c$, must also bisect $b\,h$. ∴ &c.

Cor. 4. (Fig. 4.)—Let i be the point in which the perpendicular from e meets $a\,B$, $E\,i$ is parallel to $A\,B$. For drawing $x\,I$ parallel to $A\,a$, and cutting $B\,E$ in r, this parallel is trisected in r; and from the similarity of the triangles $E\,I\,r$, $B\,x\,r$, $B\,E$ is also trisected in r. ∴ the triangles $B\,r\,I$, $E\,r\,i$ are similar. ∴ $I\,B$ and $E\,i$ are parallel.

Scholium.—In the assigned solution of the preceding problem, the base of the given segment is always one of the *extreme* sides of the triangle; and the other extreme is the chord of the arc intercepted by the trisecting lines. But if the given segment contain an angle of less than 60°, its base may be the *middle* side of an inscribed triangle, having its sides in arithmetical progression. And from what has been established, it is plain how we may construct that triangle. Let $a\,x$ (fig. 3) be given as the middle side of such a triangle, in the segment $a\,B\,x$. Then we have e, the middle point of the arc $a\,x$. ∴ we have P, the opposite extremity of the diameter $e\,c\,P$; and ∴ we have $a\,P$. But we have seen (Cor. 2) that $a\,P$ bisects the radius $B\,c$ in q. If, therefore, with the centre c, and the interval of the semiradius, we describe a circle, its occurse with $a\,P$ determines the point q. ∴ we have the diameter $B\,b$, and ∴ its ordinate $a\,A$, one of the extreme sides of the triangle; the greatest or the least, according to the point of intersection of the described circle with the line $a\,P$, through which we draw the diameter.

PROP. II. PROBLEM. (Fig. 3.)

In a given circle to inscribe a triangle, whose sides shall be in arithmetical progression, and their common difference equal to a given line.

The solution of this problem also is obvious, from what we have established in solving the former. For there we have seen that A z is the common difference of the sides, and (Cor. 2) that E N is equal to radius. Therefore, given the radius and common difference of the sides, we can construct the right-angled triangle A E N, having one of its sides inscribed in the circle, its hypotenuse E N equal to radius, and its altitude A z equal to the given common difference. This line A z produced gives the greatest side A a; whence we have the other two.

Scholium.—Whenever the given common difference is less than half the radius, we may find two different triangles to solve the problem; for either of the unequal sides, A E or A N, may be inscribed in the circle. It is plain that A z is a maximum, when equal to half E N; that is, that the semiradius is the greatest possible common difference of the sides.

Putting unity for radius, the triangle of whose sides the common difference is a maximum, is

$$\frac{\sqrt{7}+1}{2},\ \frac{\sqrt{7}}{2},\ \frac{\sqrt{7}-1}{2};$$

the cosine of whose middle angle is $\frac{3}{4}$. And it may here be briefly remarked, that in the two different triangles, whose sides have any smaller common difference, half the sum of the cosines of the middle angles is $\frac{3}{4}$. Also, the sum of the two least angles is the supplement of the sum of the two greatest; and is the *third* part of the difference between the sum of the greatest and the sum of the middle angles; just as in the triangle

$$\frac{\sqrt{7}+1}{2},\ \frac{\sqrt{7}}{2},\ \frac{\sqrt{7}-1}{2},$$

the least angle is the complement of the greatest, and is third part of the difference between the other two.

Many other and curious properties of these triangles might be stated; but we must hasten at present to other matter.

PROP. III. (Fig. 5.)

If on the same base A a there be two isosceles triangles, whose vertical angles A B a, A E a are as three to two; and from the vertex of the smaller angle lines be drawn (E T, E t) trisecting the common base; these lines will trisect (A b a) the arc of the circumscribing circle on which the greater angle stands.

Dem.—Produce the altitude E o, and let $o\,e = o$ E. Draw e A, and produce it to f. E T trisecting the altitude of the isosceles triangle E A e must bisect the leg A e in *D*. Draw D d parallel to e E; also the radius A c, cutting E T in m, and D d

in n. Lastly, from the centre c, and parallel to A E, draw c Y meeting E T in Y; which point we shall prove to be in the periphery of the circle.

From the similarity of the triangles $c\,m\,Y$, $A\,m\,E$, $c\,m : A\,m :: c\,Y : A\,E$. But we shall prove that $c\,m : A\,m :: A\,c : A\,E$; and $\therefore$ that $A\,c = c\,Y$. For the triangles $c\,m\,E$, $m\,n\,D$, being similar, $c\,m : m\,n :: c\,E : D\,n$; that is, $c\,m : \frac{A\,m - c\,m}{2} :: c\,E : \frac{A\,c}{2}$ $\therefore c\,m : A\,m :: c\,E : E\,e$. But $c\,E : E\,e :: A\,c : A\,e$ on account of the bisection of the angle $f\,A\,c$ by the line A E]. $\therefore c\,m : A\,m :: A\,c : (A\,e)\ A\,E$. But we have before seen that $c\,m : A\,m :: c\,Y : A\,E$. $\therefore c\,Y = A\,c$, and the point Y is in the periphery. But the angle $b\,c\,Y$ being equal to $b\,E\,A$, that is, to the third part of $A\,B\,a$, the line E T Y trisects the arc $A\,b\,a$ in Y. Q. E. D.

Cor. 1. (Fig. 6.)—If from p, the point where $A\,c$ meets a E, the line $p\,d$ be drawn to the middle point of A E, and be produced on each side to meet the periphery in $æ$ and q; the line $æ\,d\,p\,q$ is the side of an equilateral triangle inscribed in the circle $A\,B\,a\,b$; and if the lines $E\,æ$, $E\,q$ be produced to meet the circle $A\,E\,a\,e$ in m and n, the triangle $E\,m\,n$ is an equilateral triangle inscribed in that circle.

For the triangle $A\,p\,E$ is isosceles [for angle $p\,A\,E = c\,A\,B + B\,A\,E = B\,E\,A + 2\,B\,A\,E = 2\,B\,E\,A = A\,E\,p$]. $\therefore p\,d$ is perpendicular to A E, and $\therefore$ bisects the parallel radius $c\,x$ perpendicularly. $\therefore æ\,q$ is the side of an equilateral triangle, of which Y o is the altitude. Now drawing the line A e, it must pass through y [for $a\,A\,e = a\,E\,e = \frac{a\,B\,A}{3} = a\,A\,y$]. $\therefore A\,y$ is parallel to $p\,d$; and $\therefore E\,y$, the hypotenuse of the right-angled triangle $E\,A\,y$, is bisected by $p\,d$. But it is also bisected by Y x. $\therefore E\,y$ and $æ\,q$ bisect each other in o. $\therefore æ\,E\,q\,y$ is a parallelogram. $\therefore$ the angles $æ\,E\,c$ and $q\,E\,c$ are each of them 30° [for $æ\,E\,c = æ\,E\,y - y\,E\,b = q\,y\,E - x\,y\,E = q\,y\,x = 30°$, and in like manner, $q\,E\,c = q\,E\,y + y\,E\,b = æ\,y\,E + x\,y\,E = q\,y\,x = 30°$]. $\therefore m\,E\,n$ is an equilateral triangle inscribed in the circle $A\,E\,a\,e$.

Cor. 2.—The arc B $æ$ is the third part of $A\,æ\,B$, and B q the third part of $A\,a\,B$. For $æ\,Y\,q$ being an equilateral triangle, arc $æ\,Y = 2\,æ\,x$. But $A\,Y = 2\,B\,x$. $\therefore æ\,Y - A\,Y = 2\,æ\,B$. $\therefore$ &c. In like manner $q\,Y + A\,Y = A\,Y\,q = 2\,B\,q$ $\therefore$ &c.

Cor. 3.—If from c be drawn a parallel to A a, meeting A E in k, and Y E in l, Y l is the third part of Y E. For the triangles $c\,z\,E$ and $c\,z\,k$ are each of them isosceles. $\therefore k\,E$ = diameter. But from the similarity of the triangles $k\,l\,E$, $c\,l\,Y$, $Y\,l : l\,E :: c\,Y : k\,E :: 1 : 2$. $\therefore$ &c. It is plain that $x\,z$ must cut off another third part of E Y. Also, that $c\,k = Y\,y$.

Cor. 4.—Producing A c and A B to meet the circle $A\,E\,a\,e$ in r and s, the chord E r is equal to A a; and the semicircular arc

t E u is bisected in s. For angle A E a = E A r, the triangle A p E being isosceles. ∴ A a = E r. But arc E $t = \frac{\text{B A}}{2}$, and arc E $s = \frac{\text{A}\,e}{2}$. ∴ arc $t\,s = \frac{\text{E A}\,e}{2}$ = quadrant.

Cor. 5. (Fig. 7.)—If B f be drawn parallel to A E, it is equal to E c. For drawing $f\,g$ parallel to the axis, and meeting A E in g, $f\,g = g$ Z, on account of the equality of the angles g Z f, $g\,f$ Z. ∴ $f\,g$ + B c, or E c, = g Z + E Z = B f.

Prop. IV. (Fig. 7.)

The same things being supposed, E r the segment of the trisecting line intercepted between the two circles is the third part of E X, the segment of it included in the circle A E a e.

Dem.—Draw A n, making the angle E A n = A E x, and cutting E X in s. The triangles E A n, A E x being identical, A n = E x = E X. But E X trisecting A a, and being parallel to $a\,n$, trisects A n. ∴ A s being equal to $\frac{s\,n}{2}$, s X $= \frac{s\,\text{E}}{2}$. But A s, and ∴ s X, = $s\,r$, the angles at the base of the triangle A $s\,r$ being equal. [For angle A $r\,s = \frac{\text{A B}\,a}{3}$ = A E e. But it also equals A E r + E A r. ∴ angle E A r = X E e. ∴ angle r A s, or E A n − E A r, = A E X − x E e = A E e = A $r\,s$]. ∴ E $r = \frac{\text{E X}}{3}$.

Cor.—Drawing $a\,r\,v$, $r\,v = \frac{a\,v}{3}$. For the triangle $a\,r$ X is similar to A E a [for angle $a\,r$ X = A E a, and angle r X a = E A a]. ∵ $a\,r$ = r X, and ∴ $r\,v$ = E r. ∴ &c.

Scholium.—It is plain that A n bisects E y in o; since A o = E o, and the angle E A y is a right angle.

Prop. V. (Fig. 7.)

The same things being supposed, if there be drawn a diameter of the circle A E a e perpendicular to the axis, and meeting the circle A B a b in T, the line E T is a tangent, and equal to E t, the line drawn from E to the point where z Z produced meets the circle A E a e.

For a Z passes through k, the centre of the circle A E a e; and the triangle Z k c is isosceles, and similar to the triangle E Z c. ∴ c E : c Z :: c Z : $c\,k$; that is, c E : c T :: c T : $c\,k$. ∴ the angle c T E is a right angle, and E T a tangent. ∴ E T^2 = (c E . E k) = $\left(\frac{c\,\text{E}}{2} . 2\,\text{E}\,k\right)$ = (E d . E e) = E t^2. ∴ E T = E t.

Cor.—Hence it appears, that if c T E be a right-angled triangle, in which from the right angle the perpendicular T k is let fall on the hypotenuse; and if with c as the centre, and the side c T as radius, a circle be described, and another circle, with k as the centre and the remote segment k E as radius, intersecting the former in the points A and a; the angles A E a and A B a are as two to three. That the circles may intersect, the side c T must be more than half the hypotenuse; or the angle c E T must be more than 30°.

Prop. VI. (Fig. 8.)

The same things being supposed, the line Y x, joining the point of trisection in the arc A b a with the point in which the other trisecting line meets the circle A E a e, is a tangent to the circle A E a e, and is equal to Y y, the chord of arc $\frac{\text{A}\,b\,a}{3}$.

For the line A Y is a tangent at A, the angle a A Y being equal to $\frac{2\,\text{A B}\,a}{3}$ = A E a. ∴ A Y, or Y y, is a mean proportional between E Y and X Y, that is between E y and x y. ∴ the triangle y Y x is similar to the triangle Y E y. ∴ Y x = Y y, and is a tangent at x, the angle z x E being equal to E X x.

Cor. 1.—Let there be drawn B m, B n parallel to the trisecting lines; and let $æ$ q be the base of an inscribed equilateral triangle parallel to A a:—then the arc m $æ$ is the third part of z a r, and therefore m q the third part of z A r. For the triangles Y E y, y Y x being similar isosceles triangles, the angle E Y x (standing on the arc z a r) = E Y y − Y E y = B m n − m B n. ∴ arc z a r = n a B − 2 b n. But arc n a B = semicircular arc B b − b n. ∴ arc z a r = B b − 3 b n. ∴ $\frac{z\,a\,r}{3} = \frac{\text{B}\,b}{3} - b\,n$ $= b\,q - b\,n = n\,q$, or m $æ$.

Cor. 2.—B m is equal to y r; and y r passes through k, the centre of the circle A E a e. For arc B A m = B A Y + Y m = B a y + B r = r a y. ∴ B m = r y. But we have seen that X y = a y. ∴ k y bisects a X perpendicularly. ∴ angle a k y = $\frac{a\,k\,\text{X}}{2}$ = a E X. And angle a k e = a E A. ∴ angle y k e = A E X. But the same is the value of the angle which r y makes with the axis. For the sum of that angle and Y E b = Y r y = A E b = A E Y + Y E b. ∴ r y passes through k.

Prop. VII. (Fig. 9.)

In the produced diameter of a given circle A B a b, let E be any point whose distance from the vertex is less than radius; and from E to the extremities of the normal diameter let there be drawn the lines E D, E d, cutting the circle in the points m and n. Let k be the point in which the lines D n, d m cut the axis; and with the centre k and the interval k E let a circle be described cutting the given circle in the points A, a. Then the angle A E a is two-thirds of the angle A B a.

Dem.—Draw the radii A c, A k. The triangle c k D is similar to the triangle c D E, each being similar to d n D. ∴ E c, c D, c k are in continued proportion, that is, E c, c A, c k. ∴ the triangle c k A is similar to c A E. ∴ angle c A k = A E c = $\frac{\text{A } k \text{ } c}{2}$. But angle A c b, or A B a, = c A k + c k A = 3 c A k = 3 A E c. ∴ angle A B a = $\frac{3 \text{ A E } a}{2}$.

Prop. VIII. (Fig. 10.)

If the angles A E a, A e a be two-thirds respectively of the angles A B a, A b a, and if there be drawn from E and e the lines E T, e t, each making an angle of 30° with the axis; these lines meet each other in the periphery of the circle A B a b.

Dem.—The lines E T, e t trisect the arcs A B a, A b a respectively in T and t (see Cor. 1, Prop. III). Let the line E T meet the circle again in C, and draw the ordinate C c. The angle C B c = 60° + $\frac{\text{A } b \text{ } a}{3}$ $\left[\text{for C B } b = \text{C E } b + \text{B C E} = 30° + \frac{\text{A } b \text{ B}}{3} \therefore \text{ \&c.}\right]$. In like manner, the angle at b subtended by the ordinate drawn through the point in which e t again meets the circle is equal to 60° + $\frac{\text{A B } a}{3}$. But the two angles 60° + $\frac{\text{A } b \text{ } a}{3}$ and 60° + $\frac{\text{A B } a}{3}$ are supplemental to each other, the angles $\frac{\text{A } b \text{ } a}{3}$ and $\frac{\text{A B } a}{3}$ being together equal to 60°. Therefore, the former angle $\left(60° + \frac{\text{A } b \text{ } a}{3}\right)$ being equal to C B c, the latter $\left(60° + \frac{\text{A B } a}{3}\right)$ must be equal to C b c. ∴ the ordinate drawn through the point in which e t again meets the circle coincides with C c, and e t meets E T in C.

Cor. 1.—The sum of the angles A E e, A e E being 60° [as they are respectively the thirds of two supplemental angles], it follows that the angle E A e = 120° = E C e. Therefore a circle described with the centre c and radius c C must circumscribe the triangle E A e. And in that circle E e is the side of an equilateral triangle.

Cor. 2.—Producing A e to meet the circle A E D e in d, the angle A E d = 30° + A E a. For angle e E A = D E d, each of them being equal to C E A. ∴ angle d E a = D E c = 30°. But angle A E d = d E a + A E a = 30° + A E a.

From the principles which have been established, we should be warranted immediately to infer the following proposition. But I think it expedient to demonstrate its truth independently of the preceding theorems.

Prop. IX. (Fig. 11.)

If E *e* be the side of an equilateral triangle inscribed in the circle whose centre is *c*, and A *b* any chord intersecting it perpendicularly in *o*; assuming *o a* equal to A *o*, and drawing the radius *c a z*, I say that the angle at the periphery A E *e* is the third part of the angle at the centre *a c z*.

Draw *e r* perpendicular to E *e*, and therefore equal to radius. Then $a\,b = e\,r$ [for drawing *r s* perpendicular to A *b*, it is plain that $a\,b = o\,s = e\,r$]. Since therefore the three lines *e r*, *a b*, *c* D are equal and parallel, the lines *e c* and *r* D, *a c* and *b* D are parallel. ∴ angle $e\,c\,z = r\,\mathrm{D}\,b = \mathrm{A\,E}\,e = \frac{\mathrm{A}\,c\,e}{2}$. ∴ *e c z*, or $\mathrm{A\,E}\,e = \frac{\mathrm{A}\,c\,z}{3}$. Q. E. D.

I here dismiss the subject for the present, probably for ever; yet not without a hope that some other qualified person may be induced to pursue the train of investigation, which I have pointed out. I have hazarded the expression of my opinion, that it will yet lead to the trisection of an arc by plane geometry. I even suspect that I see how the trisection of the arc A *e d* (fig. 10) may be derived from the trisection of the arc A *b a*. But I must resign it into other hands.

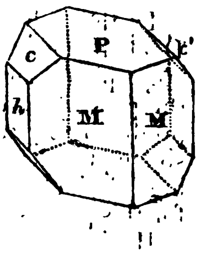
Article VIII.

On the Crystalline Forms of Artificial Salts. By H. J. Brooke, Esq. FRS.

(*Continued from* p. 288.)

Perchloride of Carbon.

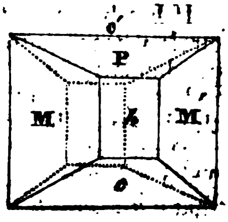
I am indebted for these crystals to Mr. Faraday. Their planes, although very brilliant while inclosed in the bottle on whose sides they have been deposited, soon become dull on exposure to the air. I have not found them cleavable in any direction, but *a right rhombic prism* may be regarded as their primary form.



P on M, or M′	90°	0′
P on *c*	119	40
P on *h*	90	0
c on *h*	150	20
M on M′	122	0
M on *h*	119	0

Muriate of Cobalt.

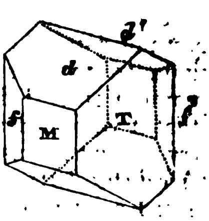
These crystals, which I have received from Mr. Cooper, may be cleaved with ease parallel to the terminal plane P, and less distinctly parallel to M and M' of the annexed figure. Hence *an oblique rhombic prism* appears to be its primary form.



P on M, or M'	109°	31'
P on *h*	122	20
P on *c'*	106	20
h on *c*	131	20
M on *h*	128	40
M on M'	77	20

Acetate of Barytes.

From the crystals of this salt with which Mr. Cooper has supplied me, it appears that the primary form is *a right oblique-angled prism.* There is a bright cleavage parallel to T, one less bright parallel to M, and an indistinct one perpendicular to these.



M on *f*	146°	18'
M on T	113	12
M on *d*	116	56
d on *d'*	126	8
T on *f'*	100	30

ARTICLE IX.

Apparatus for producing Instantaneous Light. By the Rev. J. Cumming, MA. FRS. Professor of Chemistry in the University of Cambridge.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Cambridge, April 15, 1824.*

In the early part of last December, I exhibited to our Society Doebereiner's experiment, of the inflammation of hydrogen in atmospheric air by precipitated platina; and from that time to the present have occasionally employed the same apparatus for producing an instantaneous light. The method I have used is so simple, that I should not have thought it worth communicating, if I had not been told that the instruments made in London for this purpose lose their effect after a few experiments.

Having removed the electrical apparatus from a Volta's lamp, I placed a fragment of precipitated platina between two pieces of watch spring, which are inserted into a cork fitting into a small test tube as a cap. The distance of the platina from the jet is about three-quarters of an inch, and a small wax taper is fixed about half an inch beyond the platina.

The only precautions I have found necessary are, to replace the cap immediately after each experiment, and to employ a pressure upon the hydrogen of not less than five or six inches of water. With a pressure of nine inches I have never failed in producing the same effect from platina foil of $\frac{1}{9567}$ inch in thickness, by using the precaution of keeping it in a closed tube; but when the thickness of the platina foil was $\frac{1}{6440}$, it required to be previously raised to nearly a red heat.

The experiment of the aphlogistic lamp may be exhibited to great advantage by employing either precipitated platina or platina foil suspended over the wick, instead of wire, as originally proposed. I am, dear Sir, truly yours,

J. Cumming.

P. S. By the kindness of our late Woodwardian Professor, Mr. Hailstone, I have been favoured with some fragments of titanium formed in iron slag, for the purpose of ascertaining its place in the thermoelectric series; which I find to be between platina and silver, but have not as yet been able to determine its precise situation with respect to the intermediate metals.

Article X.

On the Nuttallite, a new Mineral from Bolton, in Massachusetts.

By H. J. Brooke, Esq. FRS. &c.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *April* 20, 1824.

I HAVE received from my friend Mr. Heuland a small specimen of a mineral from Bolton, in Massachussetts, brought by Mr. Nuttall to this country.

It had been named elaeolite, I suppose from its exhibiting a play of light resembling that which is frequently seen in the fettstein from Norway, and from its colour generally approaching to that of the same mineral; some fragments are, however, nearly transparent and colourless. On examining the crystals, which are imbedded in carbonate of lime, I find that they differ both in cleavage and lustre from elaeolite, and that they are much softer. They present the form of *a right square prism* which may be regarded as the primary, with cleavages parallel to its lateral planes. The lateral edges of this prism are replaced by single planes, but the terminal planes are imperfect.

It is softer and much more glassy in its fracture than scapolite, for which, on account of its form, it might be mistaken, and does not resemble any other mineral with which I am acquainted. I have, therefore, named it Nuttallite, out of respect to the gentleman who brought it to this country. I hope at some future time to be enabled to describe it more fully than I do from the minute crystals my specimen contains.

ARTICLE XI.

Reply to Mr. Henslow's Observations on Dr. Berger's Account of the Isle of Man. By J. F. Berger, MD.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Geneva, March 18, 1824.*

BEING engaged a short time since in some researches at the public library of our city, I there met with the second part of the fifth volume of the Transactions of the Geological Society of London, the 27th article of which is entitled Supplementary Observations to Dr. Berger's Account of the Isle of Man, by J. S. Henslow, Esq. MGS. Unless I deceive myself it is the business of him whom a criticism especially concerns, either to profit by a reasonable and impartial criticism, or to animadvert upon that which is not true, or dictated by the malignity of envy.

The charge of having published a memoir from "loose memoranda,"* is of so grave a nature, that Mr. Henslow ought to have satisfied himself that it was well founded, or to have named the persons from whom he received the information.† If Mr. H. had taken the trouble to write to me upon this subject, I should have replied, without in any way discouraging a criticism upon a published memoir, that I had remained in the Isle of Man only from the 1st of June until the 8th of July, 1811, and that it was on my return from Ireland to London, in the beginning of 1813, that I drew up the memoir published in the second volume of the Transactions of the Geological Society, from notes actually made on the spot, and with the exception of Mr. Thomas Webster, Draughtsman to the Society, I have no recollection that any member of the Society ever saw these notes, for no opportunity of showing them presented itself. Mr. Webster obligingly lent me his assistance in preparing the map and sections which accompany my memoir,* but it is true that I did not consult him on subjects which were not in his department.

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* After having finished, as much as depended upon myself, the geological examination of this Isle, fearing that my notes originally written in pencil, might be obliterated, it occurred to me to fix the plumbago upon the paper by a process well known to artists, by moistening with a sponge dipped in milk, the leaves of my book, which I unsewed, and which from that time were *loose sheets.*

† *Ne quid falsi audeat, ne quid veri non audeat dicere.*

It will be readily conceived that I had not the means of ascertaining whether the southern part of the chain of mountains is placed in my map of the Isle of Man two or three miles too far to the east, according to the opinion of Mr. Henslow (p. 483). This fault may exist, and probably is not the only one. I regarded this document merely as a geological map, without absurdly pretending to dedicate it to geographical engineers.† I will only add, that, in my opinion, it is better as a topographical map of the mountainous part than those of my predecessors, and that in publishing his own, Mr. H. has not rendered mine useless, or superfluous.

I must confess I was surprised on reading (p. 482), that I had omitted to mention several formations of rocks, being persuaded that I had inspected them all, with greater or less brevity, except those which are volcanic. I was fearful that some unquestionable trace of volcanos, either extinct, or in activity, had escaped me, but I was not long in recovering from this apprehension, by observing that my critic, and I do not attach the same idea, to what is called a formation of rocks, and that Mr. H. applies this term to objects which are of very secondary importance.‡

Mr. Henslow has found granite *in situ* in other places than those which I have mentioned, and he has also traced its course further in another direction than I have done. But did this require for its narration more than three quarto pages? The rock called *greywacke*,§ by those who adopt the principles of Werner, is remarkable in this respect, that being composed of the remains of ancient rocks, it expresses a formation of modern date, which is supposed to have occurred by deposition, rather than by chemical action. Although it is not always easy to distinguish the schistose variety of this rock from argillaceous schistus, it may, however, be effected either by a glass which magnifies considerably, or by the blowpipe, which converts the greywacke into a frit only, while the argillaceous schistus fuses; this is undoubtedly the effect of the fluxes naturally mixed with it. I believe also that the mean specific gravity of this rock is rather greater than that of schistose greywacke. The natural decomposition of the latter produces a sterile sandy soil, but little suited to vegetation, on account of the readiness with which water passes through it; while, on the contrary, it is retained by the plastic property of the decomposed argillaceous schistus. Lastly, the relative positions materially assist the travelling geologist. I had carefully weighed all these distinctive characters at the time of arranging my memoir on the Isle of Man. I have allotted a considerable space to this transition rock, and I find no reason in the supplementary observations of Mr. Henslow, which induces me to alter my opinion in this respect.

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* I have publicly acknowledged my obligation to him.

† I had then only a simple pocket compass to make use of in going successively from one mountain to another.

‡ I am of opinion that in Geognosy the term *Formation* may be defined to be an assemblage of certain rocks or mineral matters, which appear, according to their geognostic relations of position, to be contemporaneous or nearly so. I am at present acquainted only with the following:

1. Formation of Primitive Rocks.
2. ———— Transition Rocks.
3. ———— Secondary Rocks (Floetz).
4. ———— Alluvial Rocks, or matter.
5. ———— Volcanos.

I leave it to those who are more capable than I am, to determine whether what is called the fresh-water limestone should be added to the most recent members of the third formation, or whether it is actually a distinct formation between the third and fourth.

§ Professor Jameson, by translating the first part of the German name into English, has rendered the pronunciation less harsh.

Mr. H. gives so indistinct an idea of the rock which characterises what he terms the "quartzose district," that I shall take no further notice of it.

I am pleased to believe that the circumscription of the limestone country is more correctly traced in Mr. Henslow's map than in mine; but the difference is not assuredly very striking. Without attaching too much value to a conjecture, I may inquire of Mr. H. how he supposes the heterogeneous substances which are found in amygdaloids have been formed there.*

I now arrive at my "great mistake" (p. 494), that of having supposed that the whole extent of the isthmus of Langness was composed of a conglomerate of quartz pebbles loosely attached together. This error on my part may, perhaps, exist, and I thank Mr. H. for having animadverted upon it;† but I am much disposed to believe, that the rock which, according to Mr. H. forms the greater part of it, ought to be called schistose greywacke, rather than argillaceous schistus.

Mr. Henslow mistakes when he makes me say (p. 495), that *all the interval* between Scarlet and Poolvash is occupied by a bed of amygdaloidal trap. I mention, on the contrary, the two places in which I had observed this rock.‡

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* It is well known that some of these substances, which are of a friable and delicate nature, often disappear from the paste in which they were inclosed, by the action of atmospheric agents.

† I owe it to truth to state, that not having completed the circuit of this isthmus, as my companions may yet remember, I had some doubts, which were removed by Mr. Webster, as to colouring my map uniformly, as has actually been done. But I ought at the same time to remark, that I do not say *in the text* of my memoir, that the isthmus of Langness is *entirely* composed of this conglomerate. I confined myself to general statements, which are not incorrect. The small extent of the isthmus of Langness, compared to that of the whole isle, ought, perhaps, to disarm the extreme severity of my critic.

‡ "Kaal-Farane and Cromwell's Walk, two places that separate Scarlet Point from the entrance of Poolvash Bay, present an unstratified bed of amygdaloid, that overlies the limestone itself."

Conformably to the general opinion, I have in my memoir given the name of *Curragh* to the flat country only, situate to the north of the group of mountains in the Isle, for I had never heard that this name was applied to the *tourbièses* of the mountainous country; the flat country which I speak of having been long used in agriculture, I thought it sufficient to state the opinion of a respectable man (Bishop Wilson), relating to the trees which they procured from it especially more than 20 years since. I have preferred to make known, by characters which are, I think, unequivocal, the use and profit that the agriculturist now derives from the marl which is also met with in this district below the sandy soil, to the subject, into the details of which Mr. H. has entered with sufficient minuteness. The discovery of elk bones and of earthy phosphate of iron in this soil is unquestionably interesting, although it does not possess any thing which is very extraordinary.

With respect to the shingly beach situate a little to the north of the sandy hills which I have marked in my map by the name of *Balla-Chirrym Hills*, Mr. Henslow exclaims against the annual increase which I assign to it, from the assertion of several inhabitants of the Isle. But has he not frequently had occasion to remark, how little the accuracy of expressions used in conversation is observed; and was not the manner of stating this assertion, which I evidenly considered as an exaggeration, sufficient to undeceive any clear-sighted person?*

I have now finished a hasty examination of a criticism, which is undoubtedly rather prolix and inflated. I should have silently profited by the truth which it may contain, if it had not been of an injurious nature with respect to me, for it tends to induce the belief that I have little respect for truth; besides which, Mr. Henslow's supplementary observations to my memoir have been published in the Transactions of a Society of which I am myself a member. *Cuique Suum.*

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* "Some go so far as to say that the increase is no less than two yards in a year." I will add that I wrote my memoir upon the Isle of Man in a foreign language, and without the assistance of any person.

ARTICLE XII.

ANALYSES OF BOOKS.

A Selection of the Geological Memoirs contained in the Annales des Mines, together with a Synoptical Table of Equivalent Formations, and M. Brongniart's Table of the Classification of Mixed Rocks. Translated, with Notes, by H. T. de la Beche, Esq. FRS. FLS. MGS. &c. London, 1824. 8vo. pp. 335, and 11 Plates.

WHEN we compare the small space of time during which geology has been pursued as a science of induction, with the quantity of information that we now possess of the structure of the globe, or at least of that portion of it which is open to the cognizance of man, we cannot fail to perceive, that the progress of geology in the mighty march of knowledge witnessed by the present century and the latter part of the preceding one, has been fully commensurate with that of the kindred sciences of Mineralogy and Chemistry. The principles of geological science have been, to a great extent, distinctly established, and are now applying to the examination of the physical structure of almost every part of the earth. The geological features of southern Europe, and of the British Isles, have been most diligently investigated,—though with respect to these alone much yet remains to be accomplished—and the knowledge thus gained has been employed with the greatest advantage, in the comparison of them with those of other countries. The results have been given to the world in a numerous series of memoirs, in almost every language of Europe, but principally in French and in German. From those in the former language, published in the Annales des Mines, a work conducted by the General Council of Mines at Paris, Mr. de la Beche has in this volume performed an acceptable service to British geologists, by selecting, with some abbreviation, a series of the more important papers. His own eminence as a geologist is a sufficient guarantee for the quality of the work: and we shall therefore confine ourselves to a brief analysis of its contents; appending to this, however, his translation of M. Brongniart's "Notice on the Magnesite of the Paris Basin," of which we purposed to have given an abstract in the *Annals*, some time since.* The work commences with Mr. de la Beche's very useful "Synoptical Table of Equivalent Formations," giving the names of the various rocks in English, French, and German, and to which are subjoined the synonymes of certain individual geologists where these differ from the terms in general acceptation. Mr. de la Beche, we observe, gives the "new red conglomerate," as the equivalent formation to the "rothe-todte-liegende," placing the latter between the alpenkalkstein (magnesian limestone) and the porphyr gebirge of Keferstein (new red porphyry, porphyre du grès rouge), which is succeeded by the coal measures: and in the preface we find the following observation on this subject:—"With respect to the identity of the new red conglomerate with the rothe todte liegende of Germany, it may perhaps be right to mention, as discussions have lately taken place on this subject between the Rev. W. D. Conybeare and Mr. Weaver, that the conglomerate usually termed new red conglomerate, in the neighbourhood of Exeter and Teignmouth, seems closely to resemble the rothe todte liegende, as has been already stated by Prof. Buckland; the magnesian limestone is unfortunately wanting in that country, or at least has not been described, though traces of it are mentioned by Mr. Conybeare (Outlines of the Geology of England and Wales, p. 308) at Sampford Peverell, in Devonshire, for beneath that rock the German rothe todte liegende is always described as occurring."

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* See *Annals*, N. S. vol. iv. p. 389.

The next article is M. Brongniart's Table of the Classification of the Mixed Rocks, from the Journal des Mines, which we are glad to see in an English dress; for, with the exception of Dr. Macculloch's, we think it the only useful arrangement of those important substances that has yet been devised: though we are also of opinion that a combination of the two might be effected, with some additions, perhaps, from M. de Leonhard's new "Charackteristik der Felsarten," which would be preferable to either. We would likewise suggest to some mineralogical geologist the propriety of determining a series of the British rocks according to the classification of M. Brongniart, and of publishing a table of their localities.

The memoirs from the Annales des Mines then succeed, and are as follows:—Geological Sketch of the Coal District of Saint-Etienne; by M. Beauier (with a geological map). Memoir on the Geographical Extent of the Formation of the Environs of Paris; by M. d'Omalius d'Halloy (with a geological map). Extract of a Memoir on the Possibility of causing Fresh-Water Molluscæ to live in Salt-Water, and Marine Molluscæ in Fresh-Water, with geological Applications; by M. Beudant. On Gabbro; by M. Von Buch. Memoir on the Mountain of Rock Salt at Cardona, in Spain; by M. P. Louis Cordier. Observations on the Formations of Ancient Gypsum occurring in the Alps, particularly on those considered as primitive; preceded by new Facts relative to the Transition Rocks of that Chain; by M. Brochant de Villiers (with a lithographic map, sections, &c.) Geological Sketch of the Thuringerwald; and on some Basaltic Mountains of Hesse and Thuringia; by M. de Hoff. Report on the Tin of Periac (dep. of the Loire Infère.); by

Messrs. Junker and Dufrenoy. Considerations on the Place that the Granite Rocks of Mont Blanc and other central Summits of the Alps ought to occupy in the Order of Anteriority of the primitive Series; by M. B. de Villiers. Memoir on the Geology of the Environs of Lons-le-Saunier; by M. Charbaut. On the relative Positions of the Serpentines (Ophiolites) Diallage Rocks (Euphotides), Jasper, &c. in some parts of the Apennines; by Alex. Brongniart (with lithographic sections, &c.). On Fossil Vegetables traversing the Beds of the Coal Measures; by the Same (with a lithographic print of the Coal Mine of Treuil, near St. Etienne, showing the Stems of large Vegetables). Notice on the Coal Mines of the Basin of the Aveyron; by M. du Bosc. Notice on the Geology of the Western Part of the Palatinate; by M. de Bonnard. On the Zoological Characters of Formations, with the Applications of these Characters to the Determination of some Rocks of the Chalk Formation; by A. Brongniart (with a lithographic print of organic remains, and another of the Montagne des Fis). Notice on the Hartz; by M. de Bonnard. On the Calcareo-trappean Formations of the southern Foot of the Lombard Alps; by A. Brongniart. Notice on the Magnesite of the Paris Basin, and of the Position of this Rock in other Places; by the Same (with a plate of sections). Observations on a Sketch of a Geological Map of France, the Pays-Bas, and neighbouring Countries; by M. d'Omalius d'Halloy (with the map). On the Geology of the Environs of St. Leger sur Dheune (dep. of the Saone and Loire); by M. Levallois.

In his table of Equivalent Formations, Mr. de la Beche has inserted the muschelkalk and quadersandstein of Germany as separate formations, in order to show the opinions at present entertained on the subject by some continental geologists, who consider the muschelkalk as distinct from our lias; and conceiving it to be of some importance to determine if we are or are not to add two new formations to our secondary rocks, he has, in an Appendix, subjoined to the above memoirs, the description of the muschelkalk and quadersandstein given by M. Humboldt in his "Essai sur les Gisement des Roches," and that inserted by Dr. Boué in his Memoir on Germany published in the Journal de Physique.

We proceed to extract M. Brongniart's notice on Magnesite:

"The distribution of the rocks and minerals entering into the composition of the crust of the globe, may be regarded in different points of view, and the different kinds of relations subsisting between these bodies successively examined.

"Sometimes we take a formation composed of different kinds of rocks, whose epoch of formation is well determined in one place, and we follow it in other parts of the globe, to see if it preserves the same position, and to study the mineralogical modifications it experiences: this point of view is *principally* geological and *secondarily* mineralogical. Sometimes we study a simple or mixed rock, of a certain nature, and following it in different places or in the different formations in which it occurs, we examine at what epochs it has been deposited on the surface of the globe, what are the minerals and rocks with which it is associated, and what peculiarities it presents in each of these epochs. This point of view is *principally* mineralogical, and *secondarily* geological: it is as productive as the first in general results, and consequently as proper as it to discover the laws which have presided at the structure of the earth, and at the formation of the minerals that enter into its composition.

It is under this last point of view that I shall consider the mineral which I have mentioned by the name Magnesite.

"The following are the minerals to which I give this name, I distinguish them in two principal series, which may one day be separated into two species when we shall have observed sufficiently essential characters to establish this distinction.

"1. Plastic magnesite (magnésite plastique), composed of magnesia, silex, and water, without carbonic acid.

"I here comprise the magnesite, so improperly named écume de Mer, that of the environs of Madrid, that of the environs of Paris, that of Salinelle, department of the Gard, &c.

"Serpentine might, from its composition, almost be referred to this species; but it is distinguished from it by its mineralogical characters.

"2. Effervescent magnesite (magnésite effervescente), essentially composed of magnesia and carbonic acid, sometimes associated with very variable proportions of silex and water.

"We may refer to this division the magnesite of Hrotubschitz, in Moravia; those of Piedmont, of the Isle of Elba, of Baumgarten in Silesia, of Styria, &c.

"Having made known, as far as it appears necessary, the minerals I include under this name, I shall now describe the position of the magnesite of the Paris basin, and present the union of a few facts and observations in order to complete the geognostic history of these minerals, the principal object of this notice.

Parisian Magnesite.

"I first observed the presence of magnesite in rather extensive beds at Coulommiers, 12 leagues to the E. of Paris, and afterwards quite close to the latter town: I shall describe this variety and the circumstances of its position with some detail, as I shall afterwards employ it as a type of comparison with the same mineral, found in other positions and in other places.*

"The magnesite of Coulommiers, in the purest specimens, for it is often mixed with other things, possess the following characters:—

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* I am indebted to M. Merimée for the knowledge of this magnesite. He was struck with the soapy unctuosity of a stone which he found at Coulommiers, and having brought it to me, he put me in the way of discovering this mineral in the Paris basin.

"Its masses are soft, smooth to the touch without being unctuous; its powder is rather hard.

"It easily absorbs water and swells out considerably, becomes slightly translucent, and forms a short soft paste, resembling jelly.

"It does not effervesce with acids.

"Exposed to the action of a porcelain furnace (at 140° of Wedgewood), it hardens, exfoliates a little, but does not suffer any other alteration; it does not show the slightest trace of fusion, either in its thin pieces or on the surface; it however becomes rough to the touch, and hard enough to scratch steel.

"M. Berthier has analyzed this magnesite, chosen from the purest masses, and has found the following ingredients:—

Magnesia	24·0
Silex	54·0
Water	20·0
Alumine	01·4
	99·4

"The magnesite of Coulommiers occurs in masses, which, by their schistose structure and thinness, show they belong to thin beds.

"Its colour is whitish, most commonly pale grey; it has often a roseate tint, but it loses that and its grey colour in the fire. It is associated with brownish and reddish chert (silex corné) of a very scaly fracture; it is intimately united with it, and penetrates into all its cavities, and even into its mass; it is also very frequently associated with marly limestone, and then effervesces and becomes partly fusible.

"This magnesite occurs in thin beds, interposed between beds of marly limestone and calcareous marl, near Coulommiers, on the right of the road, entering the town on the Paris side, in a small hill having a north and south direction, and which having been cut to form a canal, exposes its interior structure and the following series of rocks, beginning with the uppermost.

"1. A bed, composed of siliceous limestone, the middle of which is of white and cellular chert (silex corné), and the compact limestone mass filled by small shells scarcely determinable, and by larger shells, such as Limneus longiscatus, cyclostoma mumia, &c.

"2. This bed rests on a bed of very irregular thickness, of a greyish fissile earth, resembling clayey marl, and which has been recognised to be an impure magnesite, i. e. mixed with calcareous marl.

"3. Then follows a bed of soft and friable calcareous marl, containing another small bed of magnesite.

"4. A bed of calcareous marl without silex, beneath which is another small bed of brown impure magnesite.

"5. A thick bed of white calcareous marl subdivided into many strata by marl beds, and by a bed of zoned chert (silex corné zonaire), almost jaspic, without either shells or magnesite.

"6. A bed about two decimetres thick, composed of brown chert (silex corné) in irregular nodules, but principally flattened. These are the nodules that are enveloped and even penetrated by the Parisian magnesite of an isabella roseate grey colour. It is sometimes very pure, does not effervesce with acids, and is absolutely infusible in the heat of a porcelain furnace. It is sometimes slightly translucent.

"7. These cherts (silex) are placed on a bed of hard calcareous marl in nearly round nodules, and containing cyclostoma mumia.

"8. Beneath is a thick bed of white calcareous marl, friable or only splintery, and containing neither chert (silex) nor shells.

"The total thickness of the beds composing this hill is nine metres (about 29 feet).

"As this succession of beds and rocks is isolated, as no other formation is seen above it, and as we do not know that on which it rests, we can at most suspect its position by a comparison of these rocks with those that resemble them in the Paris basin; but this is a presumption difficult to prove without the presence of the organic remains found in it; now this character, which is so useful in establishing analogies between formations far distant from each other, possesses all its value when it is required to determine the position of one formation with respect to the others in the same basin; it may then be here employed with perfect safety, and geologists who admit these rules of determination, and who have seen the cyclostoma mumia and Limneus longiscatus cited, have immediately recognized the position of the formation containing the magnesite of Coulommiers. These shells are not marine, one of them is evidently a fresh water shell, consequently the magnesite belongs to a fresh water formation, and the two species of shells I have just mentioned, having as yet been only found in the middle fresh water formation, in that situated between the two marine formations of the Paris basin, we should refer the magnesite of Coulommiers to that fresh water formation; it forms part, as we have elsewhere[*] shown, of that which we have named siliceous limestone. The hard calcareous marls, and the silex that accompanies the magnesite, remind us of the siliceous and calcareous characters of this deposit, and complete all the analogies.

"The magnesite having shown itself in a very distinct manner, both as to its purity and quantity in the siliceous limestone of Coulommiers, the rules of geology teach us that we should find it elsewhere, by searching for it in this formation; this has in fact happened. Proceeding towards Paris, and at about two

[*] Description Géologique des Environs de Paris, 1822, p. 36, and 205.

leagues from Coulommiers, we observe near Crecy the same rock with the same mineralogical circumstances; i. e. the limestone so compact that it resembles the fine compact limestone of the Jura, the chert (silex), the clayey marls, the magnesite, but less pure, and the same fresh water-shells.

"The short distance of these two places rendered these resemblances very presumable; but transporting ourselves to St. Ouen, close to Paris, on the bank of the Seine and at the foot of Montmartre, we find the magnesite in a formation altogether similar to that of Coulommiers; the same limestone, the same chert (silex), the same shells occur there; the position of the rock beneath the gypsum is there well determined. The magnesite is however less pure here and less apparent; traces of it only occur; these traces had long since been observed. M. Armet had remarked the presence of magnesite in the marls of Montmartre; M. Bayen had observed, more than thirty years since, and had shown me that the menilite contained it. Now this belongs to the fresh water formation beneath the gypsum; it is probable that we should find this mineral either in minute quantities, or in small masses, in all the siliceous limestone rocks of this same formation, such as those of Champigny, Orleans, Septeuil, &c. I have recognised it in a greyish clayey marl which accompanies a silex resinite of the environs of Mans, consequently at more than 40 leagues to the west of Paris, and 50 leagues from the first place in which I have mentioned it.

"*Geological Circumstances of the Magnesite of different Places, compared with those of the Parisian Magnesite.*

"We shall find this rock still further distant, in a basin separated from ours not only by a distance of more than 120 leagues, but by chains of mountains whose structure and nature are altogether foreign to those which surround our basin; now, it is remarkable, that we find the magnesite with all the circumstances which accompany it in that part of the Paris basin where it is most pure.

"Magnesite has long since been observed at Salinelle, near Sommières, in the department of the Gard, between Alais and Montpellier; but its position has only been determined a few years since, by the description M. Marcel de Serre has published of it.

"It is therefore solely to the remarkable analogy of this position with that of Coulommiers that I wish to call the attention of naturalists. The magnesite of Salinelle is schistose like that of Coulommiers; it possesses the same colour, approaching grey with a roseate tint, with the same tenacity; it absorbs water in the same manner; it is composed of the same ingredients, i. e. 20 parts of magnesia instead of 24, 51 of silex instead of 54, and 22 of water instead of 20. It will be acknowledged that it is difficult to meet with more resemblance between crystallized minerals, which occur at more than 100 leagues from each other; and if the mineralogical species cannot be here determined by the form, it is sufficiently so by the composition; the analogies drawn from its associated minerals, and its position, are the same; it is mixed with nodules of chert (silex corné) which resemble our menilite; it is accompanied and covered by marly limestone containing fresh water shells, consequently it belongs, with that of the Paris basin, to a calcareo-siliceous fresh water formation.

"But magnesite, i. e. this stone essentially composed of magnesia, silex, and water, occurs in many other places dispersed over the surface of Europe, and consequently placed at great distances from each other. Sometimes we are acquainted with its mode of occurrence, and then we know that it is very different from that I have above described; sometimes we are ignorant of it, or at least we do but presume it; but in all these places and in all these positions we shall see the magnesite to occur accompanied by the same mineralogical characters and the same geological circumstances (circonstances geologiques)*; a consideration that must not be confounded with the geological position (gisement).

"The magnesite of Vallecas near Madrid is already known; for in 1807 I described, in my Traité de Mineralogie (t. ii. p. 492), its nature and properties, from the information obtained by the specimens received from Messrs. Sureda, Dumeril, and Mieg, and of its position from the same specimens, and the information of M. Link, who took it for a kind of clay; a very excusable error at that time. M. de Rivero has however studied the same places, and has sent me an ideal section of this rock, with a detailed description which I shall transcribe almost literally.

"'The village of Vallecus is two leagues to the south of Madrid; it is situated lower than the latter town; an isolated hill, named the hill of Vallecas, occurs near the village: before we reach the top of this hill, we meet with small hillocks and excavations which arise from the workings of the magnesite; the tour of this hill may be made in 20 minutes. From observing the locality, an idea is conceived of a gypsum basin on which the magnesian rock rests.

"'If we observe the structure of the hill, we observe, commencing at the lowest part, gypsum with clay, which belongs to the saliferous formation† of Villarubia: this gypsum extends from the walls of Madrid to the junction of the river Javama with the Manzanares; it is very distinctly seen near the hermitage of Notre Dame de la Torre, 150 metres (492 feet) to the west of the hill of Vallecas, and near the canal of Madrid; there then follows a bed of reddish clay with nodules of flint (silex pyromaque). Though the magnesite has not been observed immediately on the clay, yet M. de Rivero conceives that it rests upon it, because ascending towards the hill, the magnesite is found to follow; and the flint nodules are the same as those of the magnesite. The magnesite occurs in very thick beds, coating flints which are disseminated through the beds: these beds are cleft, and in the clefts we find asbestus (asbeste papyriforme), on which crystals of carbonate of lime are observed; they are also seen on the magnesite. This same deposit reappears close to Madrid, it may be observed as we leave the *barrière* by the Portello; the flint is there disseminated in the same manner. M. de Rivero has also met with it on the banks of the river Manzanares, opposite the king's villa; it has also been found at Cabanas, nine leagues to the north of Madrid: the author, not having visited this last place, is unable to describe its situation. A thin bed of greenish clay containing very little magnesite is observed above the magnesite at Vallecas; then follows a reddish common opal (silex resinite) in beds of variable thickness, very fragile, presenting a crust of manganese on some parts of its surface; this opal is worked for gun flints. A very soft and nearly earthy magnesite is found above this fragile opal.

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* "I have literally translated M. Brongniart's expression, though I should not have used it myself in the same sense; M. Brongniart seems only to imply that it is constantly associated with certain minerals, without any reference whatever to its geological or relative position.—(Trans.)"

† "New red or saliferous sandstone.—(Trans.)"

"'The different beds above noticed by M. de Rivero occur in the hill of Vallecas. The top of this hill constitutes a platform, on which are found many flints, and pieces of opal, with crystals of carbonate of iron; crystals of pseudo-morphous quartz have moreover been observed, and have been taken for opal crystals."

"'Shells have never been met with in this formation. The three upper beds appear on the banks on the Manzanares, as we quit the gate leading to the Escurial.'"

"The author has above stated that magnesite is met with on the banks of the river, and if we ascend towards the town, we find beds of greenish and reddish clays of which bricks are made, and above these clays an alluvial formation, composed of fine-grained sand, and lastly vegetable earth on the surface.

"Thus the magnesite of Vallecas and Cabanas, near Madrid, possesses the same tenacity, the same hardness, the same lightness, the same superficial roseate tint, as those of Coulommiers and Salinelle. It is equally composed of 23 parts of magnesia, 53 of silex, and 20 of water; it is accompanied, like ours, by chert (flint?), which also passes into its mass, by common opal (silex resinite), by chalcedony, by crystallized quartz, and calcareous spar altogether resembling those of our siliceous limestone. It affords, certainly, no organic remains; but we know that these remains are rare in the siliceous limestone of the Paris basin, of which our magnesite forms a part; lastly, if it appears to differ by its position on a saliferous gypsum, much more ancient than our gypsum, and calcaire grossier, it is not covered by any rock which appears more ancient than the latter, and it is like them in horizontal beds.

"If from Spain we transport ourselves to Italy, to the foot of the Piedmontese Alps, we shall find, at a short distance from Turin, the serpentine hills of Castellamonte and Baldissero, traversed in every direction by veins of magnesite which is tenacious yet plastic, light, and with that roseate superficial tint which we have noticed in the preceding magnesites. Its principal or fundamental and characteristic composition appears to be still the same, i. e. of magnesia, silex, and water. Here however we have carbonic acid, which seems to indicate a different chemical species; but its *geological circumstances* are still the same. I have already noticed them in my memoir on the geological position of the serpentines.

"The mineral no longer occurs in horizontal beds, or nodules interposed in the beds, but in numerous veins, uniting in every direction in the midst of the serpentine; chert, common opal, and jasper, presenting many varieties of texture and colours, are constantly and intimately united with it, as at Coulommiers and Salinelle. They have been formed even in the midst of the magnesite. This circumstance of geological association is then remarkably constant, even when the geological position has no longer the same character, and it is here very different. It appears to me well established, that this magnesite belongs to the serpentine formation of the Apennines, consequently to ancient rocks, nearly of the transition epoch.

"There are other examples of magnesites, but the circumstances of their geological position are less well known; yet both what is known, and their composition, still very well agree with what we have stated of the preceding.

"Thus the plastic magnesite of Asia Minor, known by the name of Ecume de Mer, has all the exterior characters of that of Piedmont, and even that of Coulommiers, with a composition that very slightly differs; it has, like it, the roseate superficial tint which also occurs in the magnesite of Houbricht in Moravia. But in this, the carbonic acid, which is in some quantity, seems to establish a mineralogical difference, the importance of which is not yet well appreciated; the presence of silex nodules which pass into the mass, reminds us of an analogy in the *geological circumstances*, which is rather remarkable."

Conclusions.

"We shall confine ourselves to these examples: they are sufficient to prove the relations of formation which we wish to establish between the magnesite of the Paris basin and those we have just mentioned. The magnesite in all, whether it be or be not combined with carbonic acid, contains water and silex; this last substance does not occur only in chemical combination with the magnesia, it also forms isolated masses, and whatever the mineralogical differences may be that these varieties of quartz present, not only is its difference all that is necessary to establish the geological resemblances which we desire should be remarked; but it may be said that these varieties follow without interruption from the oldest to the newest magnesites, as the following table will show:—

Parisian magnesite....	Crystallized quartz Chert Several varieties of opal (silex resinite)
Magnesite of Salinelle..	Chert
Magnesite of Madrid..	Crystallized quartz Chertz (silex corné) Chalcedony Several varieties of opal (silex resinite)
Magnesite of Moravia..	Chalcedony White and green opal (silex resinite)
Magnesite of Piedmont	Chert Chalcedony Varieties of opal (silex resinite) Jaspet

"Before geology had acquired in principles and facts the precision to which it has now arrived, the presence of magnesite in the Paris basin had no other result than that of adding a mineral species to the list of those contained in our country; but this fact now possesses another interest: it has served to unite observations which were, it may be said, isolated. It informs us that the magnesite beds were deposited on the surface of the globe at very different epochs, for some (those of Piedmont) belong to the most ancient sediment rocks, and others (those of Salinelle and Coulommiers) to the newest sediment (tertiary) rocks; and yet we see these deposits accompanied by nearly the same *geological circumstances*. Such a remarkable constancy in the association of silex and magnesia, two bodies between which there is no chemical analogy, will fix the attention of geologists, and may perhaps contribute to show us the origin of these deposits, as the thermal springs of Italy deposing travertine have pointed out that of the freshwater limestone. It is still apparently from the bosom of the earth that the liquid arose which deposited these rocks; for we find in certain thermal waters traces of all the ingredients of their composition: the mass of water is at present immense in comparison with the matters held in solution; but these matters exist in it: they are deposited, as M. Berthier has observed, at the waters of Vichy, St. Nectaire, &c.* not only separately, but nearly in the same order, as the calcareous and magnesian formations. The first deposits, those which are nearest the spring, this able chemist tells us, are also those most charged with peroxide of iron and silex; the limestone, still ferruginous, then follows, and is the more pure and more separated from these two substances, the more distant it is from the point where the spring rises from earth; the carbonate of magnesia is the last deposited.

"Without wishing to establish any real resemblance between this succession and that of our rocks; without wishing to represent that these rocks, certain beds of which show too clearly the characters of mechanical aggregation for them to have been formed by solution, have been deposited by the mineral waters of the ancient world, we cannot avoid remarking that commencing with the chalk, we find a series of rocks, the nature and succession of which are nearly the same as those which M. Berthier has observed in the deposits from mineral waters. Thus, first, a new formation, i. e. a new emission of dissolved matter would appear to commence above the chalk, at first depositing silex and iron, represented, one by the beds of sand and sandstone, and the other by the iron ore found so abundantly in the deposits of lignites and plastic clays which cover the chalk; secondly, the more or less compact limestone, accompanied by iron and silex in the lower beds, and by silex in the upper beds; the magnesite also accompanied by silex, which still occurs in the lower gypsum beds; this silex is partly soluble in alkaline liquids, like that of the calcareous deposits of certain mineral waters; fourthly, the gypsum, the most soluble substance of all those we have named, and which should be the last deposited.

"We do not pretend to draw any other conclusion from these different resemblances; but it appeared to us right to *hazard* them, if it were only to engage the attention of chemists and geologists."

From this extract the reader will be able to judge of the manner in which Mr. de la Beche has executed his task: and we will conclude by recommending this work to all students of geology; to whom it will be highly useful, by enabling them to compare our own rocks with the similar formations on the continent which are described in it.

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* Annales de Chim. et de Physique, t. xix. p. 134.

ARTICLE XIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

March 4 (*continued*).—"Some further particulars of a case of Pneumato-thorax; by J. Davy, MD. FRS."

Dr. Davy's hopes of the favourable termination of the case of Pneumato-thorax, in which tapping was resorted to, as described in the Appendix to his paper in the Philosophical Transactions for 1823,* had proved fallacious;—the patient had died; and the object of the present paper was briefly to detail the progress of his disorder, and to give the examination of the air found in the chest.

About a month after the date at which the history of the case in the Philosophical Transactions terminates, hydrothorax supervened, and it was likewise found that air was collected in the left cavity of the chest. A consultation being held upon the case, a second operation was determined on. Dr. Davy having experienced inconvenience in penetrating the intercostal space, adopted the method of perforating a rib, mentioned by Hippocrates. Part of the fifth rib was accordingly laid bare by the scalpel, then bored through by a carpenter's auger, and the pleura penetrated by a trocar: about fourteen ounces of clear fluid were obtained, containing albumen, and a little sub-carbonate of soda, but no free carbonic acid; the succeeding portions, however, were more and more purulent, and contained gas. The total quantity of fluid thus obtained in the course of six weeks amounted to twenty pints. By means of a trocar and bladder air was obtained from the aperture at three several times; and being examined by lime water and phosphorus was found to consist of from 88 to 90 per cent. of azote, 2 to 4 carbonic acid, and 3 to 5 oxygen. The patient was at first much relieved by the operation, and seemed to be recovering: but he eventually became worse, and died; evidently from the mere effects of the disorder.

On the examination of the body after death six ounces of pus were found in the right pleura; the right lung at first appeared healthy, but upon minute examination a number of granular transparent tubercles were found disseminated through it. The left lung was much condensed, so that it could not be inflated by blowing with a pair of double bellows attached to the trachea; it communicated with the pleura by two small openings. The heart was displaced, having been thrown to the right side, obliquely on the spine. The body having been opened in a bath, 170 cubic inches of gas were collected from it, containing 16 per cent. of carbonic acid, and a little oxygen;

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* See our last number, p. 302.

the residue being azote. This, Dr. Davy presumed, to be atmospheric air, deteriorated by respiration, and altered by the absorption it had undergone while in the body. He had found in the lungs, after death, in various cases, from 6 to 12 per cent. of carbonic acid.

March 11.—A paper was read "On the Parallax of α Lyræ" by J. Brinkley, DD. FRS., &c. In this paper Dr. Brinkley wholly opposes and controverts the statements of Mr. Pond, respecting the subject of his paper, as given in the Phil. Trans. for 1823, and noticed in the *Annals* for September last, p. 226.

March 18.—The Lord Bishop of Limerick was admitted a Fellow of the Society; and the name of the Earl of Oxford was ordered to be inserted in its printed lists.

A paper was read, entitled, "An Account of Experiments on the Velocity of Sound, made in Holland. By Dr. G. A. Moll, and Dr. A. Van Beck."

This paper commences with some observations on the Newtonian formula for the velocity of sound, as modified by Laplace: and the authors then proceed to consider the effect of the wind on that velocity; which, in their own experiments, they contrived to annihilate. These experiments were made on the plains of Utrecht, at two stations 9964 feet distant from each other; and the velocity ascertained by determining the interval between the flash and the report of guns by means of clocks with conical pendulums, dividing twenty-four hours into 10,000,000 parts. The states of the barometer and thermometer were noticed, and the humidity of the atmosphere determined by means of Daniell's hygrometer. The general result is, that at the temperature of 32° the velocity of sound is 1089·7 feet per second. Various detailed tables of the experiments and attendant circumstances are annexed to the paper.

March 25.—Major-General Sir John Malcom, GCB. was admitted a Fellow of the Society; and a paper was read, on the Geological Distribution of Fossil Shells, in continuation of that already published in the Phil. Trans.* by L. W. Dillwyn, Esq. FRS.

A letter from Thomas Tredgold, Esq. Civil Engineer, to Thomas Young, MD. For. Sec. RS. was likewise read: it contained an account of a series of experiments on the elasticity of steel at different degrees of temper; describing the apparatus with which they were made, and giving their various results.

April 1.—The reading was commenced of "An Inquiry respecting the nature of the luminous power of some of the Lampyrides; *L. splendidula* or Glow-worm, *L. Italica*, or Fire-fly, and *L. noctiluca*: by Tweedie John Todd, MD.: communicated by Sir E. Home, VPRS."

April 8.—The reading of Dr. Todd's paper was resumed and concluded. This paper commences with some general remarks on the various causes to which the luminosity of the lampyrides has been ascribed; the explanation of Macartney and Macaire, that the light they emit is a simple product of vitality, being considered as the true one. Dr. Todd then proceeds to a minute account of the apparent source and characters of the light in the several animals; describing the manner in which its emission is affected by solar and other light, by heat, and by certain chemical agents respectively. In the *Lampyris splendidula*, the light is of a fine topaz yellow colour, with a tinge of green, and is extremely vivid within the compass of a few inches, but does not extend its brilliancy far around: within that space the hour may be seen on a watch by its means. The light of the Fire-fly is of a pale yellowish tint, with continual flashes of vivid light: its variations are not connected with the motions of the insect's wings, nor are they produced, as some have affirmed, by the frequent intervention of a membrane. This animal may be seen shining in full moon-light; which is not the case with its congeners. Irritants excite the luminous power in all cases, and disorganizing substances destroy it. Dr. Todd concludes that this power is solely an effect of vitality, and that the light may be considered as *animal light*; being analogous to animal heat, which arises from a power of separating heat from its combinations with matter. He adopts the hypothesis that its principal use is that of guiding the male insects to the female, in the season of sexual congress: the males always approach any light; and sometimes even the shining females of other species, until they come very near them. The fact that the larvæ and even the ova possess a degree of the luminous faculty, Dr. Todd does not consider as militating against this explanation; for various organs are partially developed in the earlier stages of many animals, which are only to be used by them when arrived at their perfect state.

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* See *Annals* for March, p. 177.

A paper was also read, entitled, "A Comparison of the Barometrical Measurement of Altitude with that by Trigonometry: by Capt. Edward Sabine, FRS."

This paper contains the details of a comparative measurement of the height of an hill at Spitzbergen in July last, by the geometrical and barometrical methods: the instruments employed in both operations, and in the latter especially, had been prepared with more than ordinary care, and the observations were conducted with an attention to every circumstance which, it was conceived, might influence the strictness of the comparison, and sufficiently repeated to diminish at least the slight but unavoidable errors of observation. In the geometrical determination, the base, exceeding 2000 feet, was measured on the frozen surface of a bay at the foot of the hill, from whence a polished copper cone fixed on the summit was visible: the horizontal and vertical angles were observed by a repeating circle;

the height thus found was 1643 feet. The barometers were made under the inspection of Mr. Daniell, with iron cisterns, as described by Mr. Newman the maker, in a recent number of the Quarterly Journal of the Royal Institution*; the one conveyed to the top of the hill was stationary there several days, and repeated observations were made on each; the mean height deduced from them was 1640 feet and a fraction, being less than three feet in defect, when compared with the geometrical measurement. The height is deduced from the barometrical observations by the method given by Mr. Daniell, in the Quarterly Journal.

The near accordance of these results will, Capt. S. hopes, be satisfactory to those who are practically acquainted with the very ready means which the barometer affords of measuring heights; the doubt which had been thrown on its equal applicability in the northern regions, as in the temperate and tropical climates, by the great differences which appeared in a similar comparison made by Capt. Phipps and Dr. Irving, in the year 1773, and which are now shown to *have* originated in error of some kind, being wholly removed.

The Society, on account of the approaching festival, then adjourned over two Thursdays, to meet again on the 29th of April.

LINNEAN SOCIETY.

Dec. 16, 1823.—The reading of Mr. Murray's paper on the *Lampyris noctiluca* was resumed and concluded; and the following communications were read.

"Observations on some of the terrestrial Mollusca of the West Indies; By the Rev. Lansdown Guilding, BA. FLS." Among the species described in this paper were *Helicina occidentalis*, corpore livido, dorso tentaculisque atris, oculis prominulis.—In montibus sylvosis Sancti Vincentii; *Bulimus hæmostomus*, corpore olivaceo-nigro, corrugato: pede subtus pallido: capite bifariam crenato.—In dumetis Antillarum; *Bulimulus stramineus;* and *Pupa undulata.*

"An account of some rare West Indian Crabs;" by the same. The Society then adjourned to January 21, 1824.

Jan. 21.—Among the presents received at this meeting was a specimen of a new species of *Cyprinus viviparus*, from Don Vincente de Cervantes, Professor of Botany in the University of Mexico.

A paper was read, "On a new species of the genus Gadus: by Mr. Jonathan Couch of Polperro, in Cornwall." This diminutive species, called by fishermen the Mackarel Midge, is only an inch and a quarter in length: its proportions are nearly those of the Whiting.

The reading was commenced of a paper "On the Natural

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* Mr. Newman's account of these instruments will be found in the last number of the *Annals.*

Affinities that connect the Orders and Families of Birds. By N. A. Vigors, Esq. M.A. F.L.S. Communicated by the Zoological Club of the Linnean Society."

Feb. 3.—Among the presents received at this meeting was a Collection of Plants, made by Lieut. Col. Wright, of the Royal Engineers, during a journey through Circassia, Persia, and Georgia.

A notice by Mr. John Hogg, of Norton, Durham, was read, stating that a fine specimen of *Falco chrysaëtos*, or Golden Eagle, was lately shot near the mouth of the Tees; being the fifth known to have been killed in England.

The reading of Mr. Vigors' extended paper was then resumed and continued; and it likewise occupied the attention of the Society on *Feb.* 17 and *March* 2.

March 16.—The reading of Mr. Vigors' paper was also continued at this meeting; and the following other communications were read.

"Description of *Erythrina Secundiflora*. By Don Felix Avellar Brotero, Emeritus Professor of Botany in the University of Coimbra; For. Mem. of the Society."

"On the insect called *Oistros* by the ancient Greeks, and *Asilus* by the Romans. By W. S. MacLeay, Esq. FLS. Communicated by the Zoological Club of the Linnean Society." In this paper, which may interest the lovers of classical antiquity as well as of natural history, Mr. MacLeay has produced many interesting proofs that the *Œstrus* of the ancients,

> ——— cui nomen *Asilo*
> Romanum est, *Œstron* Graii vertêre vocantes." (Virg. Geor. II.)

was not the insect to which the name is now given; but a *Tabanus*. Olivier first observed that it was different from the *Œstrum* of the moderns. Pliny uses the name *Tabanus* or the Μύωψ, which Aristotle says is nearly related to *Œstrus*, both being ἐμπροσθόκεντρα; it cannot therefore be the modern *Œstrus*: he also says that both are bloodsuckers, which agrees with the Linnean *Tabani*, but is wholly inapplicable to the modern *Œstrus*. As the insect is too well known for its name to have been forgotten or misapplied, there can be little doubt that the Latin *Tabanus*, the Italian *Tabano*, Spanish *Tavano*, and French *Taon* are identical, which latter name Mouffet gives us the same with the English Breese, Clegg and Clinger, mentioned by Shakspeare, who, speaking of Cleopatra, says:

> "The Brize upon her, like a cow in June,
> Hoists sail and flies."

Some elucidation is also brought from Homer, and the Prometheus of Æschylus, and it is observed that Virgil describes the *Asilus* or *Œstrus* as abundant and *acerba sonans*, whereas our *Œstrus bovis* is a rare and silent insect. They were first confounded by Valisnieri, who has been followed by Martyn and others. It is inferred that Aristotle did not even know the latter, from his assertion that no dipterous insect has a sting behind.

April 6.—A letter was read, from the Rev. W. Whitear, of Harleston, in Norfolk, stating that a Little Bustard had been shot, in December last, at Little Clarton, in Essex. He considers it to be a curious fact that this bird, an inhabitant of a southern climate, should have been met with in this country, in winter.

A description was likewise commenced, of a Collection of Arctic Plants formed by Captain Sabine, during a voyage to the Polar Seas, in 1823: by W. J. Hooker, LLD. FRS. &c. Communicated by the Council of the Horticultural Society.

April 20.—Sir T. Gery Cullum, Bart. FLS. presented some sections of Fir timber, pierced to a great depth by the *Sirex juvencus* of Linnæus; together with specimens of the insect itself. They were from the woods of Henham Hall, in Suffolk, the seat of the Earl of Stradbroke, where two hundred Scotch Firs have been destroyed by this insect; being bored through and through. The reading of Dr. Hooker's description of the Arctic Plants collected by Capt. Sabine was continued.

A Catalogue of the Norfolk and Suffolk Birds, with remarks; by the Rev. Revett Shephard, AM. FLS., and the Rev. W. Whitear, AM. FLS., was read in part, and the remainder postponed to a future meeting.

ZOOLOGICAL CLUB.

We have hitherto been prevented from noticing this useful association. Its first meeting was held in the apartments of the Linnean Society on the 29th of November last, the birth-day of our celebrated countryman John Ray. The Club is composed of members of the Society devoted to the study of zoology and comparative anatomy, and has been organized with the view of advancing the knowledge of those sciences, in all their branches, under the sanction of the Society. This body will not have any publications of its own, but will submit all original communications made to it to the Council of the Linnean Society, who will decide upon them as upon all other communications.

Before the Zoological Club proceeded to the election of their officers and the other business of the day, an admirable opening address, explanatory of the views of the association, was delivered by the Rev. W. Kirby, FR. and LS. who had been unanimously called to the chair.

The following members were then appointed to form the Committee and Officers for the management of the affairs of the Club for the ensuing year:—

Joseph Sabine, Esq. *Chairman;* J. F. Stephens, Esq. *Treasurer;* N. A. Vigors, Esq. *Secretary:* Rev. W. Kirby; A. H. Haworth, Esq.; Thomas Horsfield, MD.; Thomas Bell, Esq.; E. T. Bennet, Esq.; G. Milne, Esq.

The meetings of the Zoological Club, at which all the members of the Linnean Society are entitled to be present, are held at the Society's apartments in Soho Square, at eight o'clock in the evening, on the second and fourth Tuesdays of every month throughout the year.

ASTRONOMICAL SOCIETY.

March 12.—The papers read at this meeting of the Society were as follows:

A letter from Sir Thomas Brisbane, Governor of New South Wales, to F. Baily, Esq. accompanied by Mr. Rumker's observations of the Summer Solstice 1823 at Paramatta; the results of which are:

For the mean obliquity of the Ecliptic.	23°	27′	44·39″
For the latitude of the place of observation. ..	33	48	42·61

Also the mean of twelve months' meteorological observations made at Paramatta between May, 1822, and May, 1823.

A letter from Prof. Schumacher, of Altona, including Mr. Hanson's computations of the elements of the comet of 1823, 1824, from observations made in the month of Jan. 1824.

Two letters from Mr. Taylor, jun. of the Royal Observatory, Greenwich; the first containing the elements of the same comet as computed by himself from the Greenwich observations of January, 1824, using Boscovich's method; and the second, a comparison of anticipatory ephemerides of the places of this comet, from the elements computed severally by Schumacher, Carlini, Dr. Brinkley, and himself, with the Greenwich observations.

On the Rectification of the Equatorial, by J. F. Littrow, Director of the Imperial Observatory at Vienna. In this paper the author directs his attention to those errors only which depend upon the placing and use of the instrument, which the observer himself must either be able to obviate or allow for; and he therefore enumerates the greater part of them, and points out means for their rectification.

On the Utility and probable Accuracy of the Method of determining the Sun's Parallax, by observations on the planet Mars near his opposition; by Mr. Henry Atkinson, of Newcastle-upon-Tyne. In this paper the author shows, that in a series of observations on Mars, taken with good instruments used in north and south latitudes, the probability of error is very small; and as the synodical revolution of Mars takes place in about 780 days, that planet will be 23 times in opposition before the next transit of Venus on the 8th Dec. 1874. Hence he infers, that if careful corresponding observations are made on each of those 23 oppositions, the probable error would be reduced nearly 4·796 times. The author concludes his paper by describing what he regards as the best means of carrying this method into effect.

A new annular Micrometer by Frauenhofer was submitted to the inspection of the Meeting by Mr. Francis Baily. This instrument is called by the artist the *suspended* circular micrometer, from the circumstance of its appearing (in the telescope) as if suspended in the heavens without any support. It consists, in fact, of nothing more than a circular piece of plate-glass about one-inch in diameter, in the centre of which a circular hole is cut, of half an inch in diameter. To the inner edge of this glass circle a narrow ring of *steel* is firmly and securely fastened; and, the whole being put in a lathe, the steel ring is turned perfectly circular, and reduced to a very *thin* edge, both at its exterior and its interior circumference. The glass, with its steel circle, is then burnished into a brass ring or cap, by means of which it may be placed, when required, in the focus of the telescope.

The advantages attending this construction are, 1. The preservation of the circular form of the ring, as it comes from the lathe, without the risk of its being injured in attaching it to the telescope in the usual manner: 2. In the use of steel instead of brass, whereby a finer edge may be given to the circumferences: 3, In rejecting the metal arms by which these rings were formerly attached to the sides of the telescope, from the unequal expansion of which (or any external violence given thereto) the perfect form of the circle might be injured, without being immediately detected: 4, In thus avoiding the obstructions which those arms might, in some cases, by their position, occasion in the observations of the passage of a star before it entered the interior of the ring.

April 9.—At this meeting the following papers were read, viz.:

1. On the Elements of the Orbit of the Comet of 1823, computed from Observations made at the Royal Observatory at Greenwich, by Mr. W. Richardson, Assistant to the Astronomer Royal. These elements were computed by Dr. Olber's method. The paper likewise contained a comparison of his elements with the Greenwich observations from Jan. 1 to Feb. 2, and in more than half the observations, the results of the elements did not differ from them so much as $2'$ in longitude, or so much as $1'$ in latitude.

2. On the Corrections requisite for the Triangles which occur in Geodesic Operations; by Capt. G. Everest, of Bengal, Conductor of the Trigonometrical Survey in India. This paper contained the solution of two problems by formulæ employed in India since 1819, and which the author thinks preferable to those given by M. Delambre for the same purpose. They require the use merely of pocket logarithmic tables, with four places of decimals, of which copious examples were given; and

the paper concluded by the application of these formulæ to the corrections of angles actually observed in the operations in India.

3. On the Method of determining the Difference of Meridians by the Culmination of the Moon; by Francis Baily, Esq. FRS. V. Pres. Ast. Soc. This paper was too long to permit its reading to be completed at the present sitting; and we shall, therefore, reserve our remarks upon it until it is concluded.

Several very valuable books were presented to the Society.

GEOLOGICAL SOCIETY.

Feb. 20.—A notice was read on the Megalosaurus, or great Fossil Lizard of Stonesfield, near Oxford; by the Rev. W. Buckland, FRS. FLS. President of the Geological Society, and Prof. of Mineralogy and Geology in the University of Oxford, &c. &c.

The author observes that he has been induced to lay before the Society the accompanying representations of various portions of the skeleton of the fossil animal discovered at Stonesfield, in the hope that such persons as possess other parts of this extraordinary reptile may also transmit to the Society such further information as may lead to a more complete restoration of its osteology. No two bones have yet been discovered in actual contact with one another, excepting a series of the vertebræ. From the analogies of the teeth they may be referred to the order of the *Saurians* or Lizards. From the proportions of the largest specimen of a fossil thigh bone, as compared with the ordinary standard of the Lacertæ, it has been inferred that the length of the animal exceeded forty feet, and its height seven. Prof. Buckland has, therefore, assigned to it the name of Megalosaurus. The various organic remains which are found associated with this gigantic lizard form a very interesting and remarkable assemblage. After enumerating these, the author concludes with a description of the plates, and observations on the anatomical structure of such parts of the Megalosaurus as have hitherto been discovered.

MEDICO-BOTANICAL SOCIETY.

Feb. 13.—Some observations were made on the Acacia Catechu. A paper was also read, on a bark termed the Malambo Bark, lately imported from America.

Feb. 27.—Some observations were read, on the alterations in the Pharmacopœia.

March 12.—A paper was read, entitled, "Observations on the Anthroxanthum Odoratum; by T. Rowcroft, Esq. his Majesty's Consul General at Peru; communicated by Dr. Bree, President.

March 26.—Some observations were made on the Croton Tiglium; by Mr. Pope, of Oxford-street.

April 9.—A paper was read on the Resina Acaroides, by Mr W. Bollaert.

ARTICLE XIV.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *The Logan Stone in Cornwall overturned.*

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Plymouth, April 18, 1824.*

Your geological readers will hear with infinite regret, that the celebrated *Logan Stone* in Cornwall, which has for so long a period been regarded as an object of great national interest and curiosity, and which has been visited by persons from the remotest extremity of Europe, has within the last few days been overturned by *one of the Lieutenants of his Majesty's navy*, now commanding a revenue cutter, stationed between the Lizard and Lands End, assisted by a party of his men. The barbarous and wanton folly which could induce an officer bearing his Majesty's commission to commit so unwarrantable an act, as to remove a great national curiosity from a position in which it had stood for ages, defying the hand of time, and affording to the enlightened traveller an object of such singular interest, will, it is hoped, be visited with the severest displeasure of the Admiralty. In a tour through Cornwall in the summer of 1821, I was informed by a cottager who lived near the spot, that an attempt was made by a party of seamen some years before, to remove it, but without success. Cornwall, by this wanton outrage, has lost one of its most interesting monuments.

I remain, dear Sir, yours very truly, G. W. HARVEY.

II. *The Rate of a Chronometer varies with the Density of the Medium in which it is placed.*

Mr. Harvey, FRSE. has lately discovered that the density of the medium in which a chronometer is placed, has a sensible influence on its rate, *in most cases producing an acceleration, when the density is diminished, or a retardation, when the density is increased.* In a few time-keepers he has found the reverse to take place, viz. *a decrease of rate from diminished density, and an increase from increased density*; but the former appears to be the most general effect. Mr. Harvey has proved this to be the case, by an extensive course of experiments, and in which he has subjected many chronometers to pressures, from half an inch of quicksilver to 75 inches; and in all cases has found, that if a time-keeper *gained* by *increasing* the density, it *lost* by *diminishing* it, and *vice versâ*. A difference of density denoted by an inch of quicksilver, is sufficient to produce in many chronometers a visible alteration of rate.

The following are a few of Mr. Harvey's results:—

A pocket chronometer which possessed a steady rate of + 1″·9 under the ordinary circumstances of the atmosphere, had its rate increased to + 6″·2, when the density of the air was diminished to a quantity represented by 20 inches of quicksilver; and on afterwards placing it in air of a density denoted by 10 inches of quicksilver, a further increase of its rate to + 11″·9 took place. On restoring the timekeeper to the ordinary circumstances of the atmosphere, its rate returned to + 2″·1.

In another set of experiments with the same chronometer, Mr. H. placed it in a condenser, under an atmospheric pressure of 45 inches, when its rate changed to $-4''\cdot4$; and on increasing the density of the air to a quantity denoted by 60 inches of mercury, the daily variation farther declined to $-8''\cdot2$.

In another remarkable experiment, Mr. Harvey found, that when the rate of a chronometer was $+23''\cdot5$, under a receiver having its air exhausted to a quantity denoted by half an inch of mercury, the rate was altered to $-17''\cdot2$, when the air was increased to a density corresponding to 75 inches of quicksilver; the rate of the time-keeper, under the ordinary circumstances of atmospheric pressure, being $+4''\cdot7$.

Mr. H. has, we understand, drawn from it several important conclusions. For example, that a chronometer constructed in London, nearly on the level of the sea, would undergo an alteration of rate, from difference of atmosphere alone, if transported to Geneva, to Madrid, to Mexico, or any other place, situated much above the level of the place where it was constructed.

III. *Cheltenham Water.*

Mr. Faraday has examined the water from the Orchard well at the above place. A pint of this water yielded:

Carbonate of lime	1·6
Sulphate of lime	14·5
——— magnesia	12·4
——— soda	3·7
Muriate of soda	97·0
	129·2

Besides which the water contained a portion of carbonic acid; and a small quantity of peroxide of iron had settled at the bottom of the bottle. By using two tests suggested by Dr. Wollaston, this water was also found to contain small portions of nitric acid and potash.

On adding sulphuric acid to a portion of this water, in quantity abundantly sufficient to decompose all the salts subject to its action, and boiling the acidulated water in a flask with a leaf of gold for an hour, the gold either in part or entirely disappeared, and a solution was obtained which, when tested by protomuriate of tin, gave a deep purple tint. Hence the presence of nitric acid, originally in the water, was inferred, and that no mistake might occur, a solution made in pure water of all the salts, except the nitrate found in the water, was boiled with some of the same sulphuric acid, and tested by the same muriate of tin; but in this case no colour was afforded, nor any gold dissolved.

The potash was ascertained to be present by evaporating a quantity of the water until reduced to a small portion, filtering it, and then adding muriate of platina in solution. Three pints of the water, evaporated until about an ounce of fluid remained, gave an abundant precipitate of triple salts of potash and platina. In cases where small quantities of the waters were tried, it was necessary to let the liquid stand an hour or two after applying the muriate of platina, but the triple salt always ultimately appeared.—(Royal Institution Journal, vol. xvii. p. 179.)

IV. *Detonating Silver and Mercury.*

Dr. Liebig has analyzed both these compounds, prepared by the well-known process of causing alcohol to act upon the nitrates of the respective metals. It appears from the experiments detailed that the substance combined with the metallic bases is an acid, and separable from them by means of the alkalies and metals, and they then form the detonating compounds. To analyze detonating silver and mercury, 100 parts of each were mixed with 400 parts of calcined magnesia, and heated in a retort, the products received were:

	From detonating silver.		From detonating mercury.
Carbonic acid	35·5		25·8
Ammonia	13·7		10·0
Water	7·2		5·2
Silver	41·0	Mercury	56·9
Loss	2·6		2·1
	100·0		100·0

The above are the mean of four experiments; these give as the ultimate elements.

	Detonating silver.		Detonating mercury.
Oxygen	32·22		23·39
Hydrogen	3·22		2·34
Azote	11·28		8·23
Carbon	9·68		7·04
Silver	41·00	Mercury	56·90

The salts formed of the acid of these detonating compounds have been termed *fulminates*. With potash, the salt formed crystallizes in long brilliant plates, which do not affect turmeric paper, have a disagreeable metallic taste, and detonate when heated or struck. It consists of 85·08 acid, and 14·92 base. The fulminate of soda crystallizes in brown brilliant plates, it is more soluble in water than the fulminate of potash, but resembles it in other properties, and is composed of 88·66 acid, and 11·34 of soda. Magnesia, barytes, strontia, zinc, and copper, all combine with this acid to form compounds.—(Ann. de Chim. xxiv. 294.)

V. *Absorption of Air by Mercury.*

In our analysis of Sir H. Davy's paper "On the Electrical Phenomena exhibited in vacuo," *Annals*, N. S. iv. 379, we briefly mentioned his statement respecting the absorption of air by mercury, and its emission when the mercury is heated in vacuo. In Mr. Daniell's Meteorological Essays, p. 363, we find a different view of the subject; and as it is one of considerable importance, we now present that view to our readers.

"During my experiments upon the filling and boiling of the barometer tubes, my attention was particularly directed to the assertion of Sir H. Davy (Phil. Trans. 1822, p. 74), that 'there is great reason to believe that air exists in mercury, in the same invisible state as in water, that is, distributed through its pores;' and to the disheartening fact (if proved), that absorption of air 'may explain the difference of the heights of the mercury in different barometers; and seems to indicate the propriety of re-boiling the mercury in these instruments, after a certain lapse of time.' It is with much diffidence that I am compelled to differ from the high authority of the President upon this interesting point: but there is one observation which I made, which, I think, nearly disproves the supposition. All fluids, which are known to absorb air into their pores, invariably emit it when the pressure of the atmosphere is removed; but upon an attentive examination of large bodies of mercury, variously heated in the vacuum of an air-pump, I never saw a bubble of air given off from the surface of the metal. Air will rise from the contact of the mercury with the glass in which it is contained, in exact inverse proportion to the care with which it has been filled, but it never rises from the surface of the mercury alone. The difficulty of properly filling a barometer tube, I attribute to the attraction between the glass and the air, not to that between the mercury and air; and I believe that air will insinuate itself a little way between the glass and the metal, at the exposed end of a boiled tube, but that this cannot happen if the end be plunged in mercury; and, consequently, that no deterioration of barometers is to be apprehended from this cause. Such a deterioration, indeed, if it had existed, must, long ago, have been detected from the instruments themselves; for although the register of the Royal Society is not in such a state as to enable any one to reason upon its conclusions, that of the Royal Observatory of Paris, and some others, must have disclosed the fact."

VI. *Connexion of Phosphorescence with Electricity.*

The sulphate of quina was shown by M. Callaud d'Annecy some time since to become highly phosphorescent when rubbed at a temperature of 212°. MM. Dumas and Pelletier have ascertained, that it becomes highly negatively electrical when rubbed on woollen cloth, and hence were led to the verification of a suspicion they had long entertained, that phosphorescence was an electrical phenomenon. About two or three ounces of sulphate of quina were introduced into a glass flask, and heated for half an hour in a water bath at 212° F., it then by friction gave out a sufficiently intense light. The flask was closed by a cork, through which passed a wire pointed at the inner extremity, and terminated by a ball at the external end; on approaching this ball, two or three times to the knob of a voltaic electrometer furnished with its condenser, having taken care to shake the flask before each contact, the leaves became so electrical as to diverge as much as the instrument would admit of, the electricity being constantly positive.

The sulphate of cinchonia, which is phosphorescent like the sulphate of quina, though less so, also became electrical in the same manner. Its electricity, though of the same kind, was not so strong as that of the preparation of quina.—(Ann. de Chim. xxiv. 171.)

VII. *Preparation of Oxide of Nickel.* By M. Berthier.

Speiss, or impure nickel, is to be reduced to fine powder and roasted till it gives off no further vapours of arsenic, the heat being at first moderate to prevent fusion, and then increased. Metallic iron in the state of filings or nails is to be added in a quantity which ought previously to be determined, and the whole dissolved in boiling nitro-muriatic acid, so much nitric acid being used that no protoxide of iron remain in the solution; evaporate to dryness and re-dissolve in water, when a large quantity of arseniate of iron will be left. Add to the solutions successive portions of carbonate of soda until a greenish precipitate appears, at which time all the arsenic and iron will be separated, and part of the copper; the rest of the copper may be separated by sulphuretted hydrogen, and the clear solution thus obtained, when boiled with sub-carbonate of soda, yields the carbonate of nickel.

Thus obtained, the carbonate of nickel contains a little cobalt; to separate the latter, the precipitate, as obtained above by boiling with sub-carbonate of soda, is to be well washed and diffused whilst moist in water, and a current of chlorine passed into it until in excess: the excess of chlorine is to be allowed to dissipate and the solution filtered; it now contains not the smallest trace of cobalt, that remaining as a hydrated peroxide, with a certain portion of nickel in the same state. If in the mixed carbonate of nickel and cobalt, the latter is in excess; the residue, after the action of the chlorine, is pure hydrate of cobalt, and the solution contains the nickel with a small quantity of cobalt.—(Ann. de Chim. xxv. 95.)*

VIII. *Prussian Blue.*

Mr. Badnall, of Leek, has taken out a patent for improvements in dyeing with Prussian blue. The improvement consists in preparing the Prussian blue, by mixing it in fine powder with strong muriatic acid, and stirring it until the whole becomes a smooth homogeneous mass of a semi-gelatinous consistence. We notice it here merely to remark on the circumstance that an agent in which Prussian blue is insoluble, should be found useful in enabling it to combine with silk, cotton, wool, &c. The pure ferro-prussiate of iron is soluble in water, but the addition of a small portion of muriatic acid immediately precipitates it; wash away the acid by pure water, and the pigment becomes soluble again; re-acidify, and it re-precipitates.—(Institution Journal.)

ARTICLE XV.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

An Appendix to Capt. Parry's Second Voyage of Discovery, containing the Natural History, &c. 4to.

The private Journal of Capt. G. F. Lyon, of his Majesty's Ship Hecla, during the recent Voyage of Discovery, under Capt. Parry, 8vo.

Narrative of the Proceedings of the Expedition to explore the Northern Coast of Africa, in 1821, 1822. By Capt. W. F. Beechey, RN. and H. W. Beechey, Esq. 4to.

Narrative of Four Voyages of Survey in the Inner Tropical and Western Coast of Australia, between the Years 1817 and 1822. By Philip Parker King, RN. Commander of the Expedition. 4to.

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* Dr. Thomson's process for obtaining pure oxide of nickel is much more simple, and I believe as efficacious as the above.—*Edit.*

Narrative of a Voyage of Discovery in the Interior of Africa, from the Western Coast to the River Niger in 1818, 1819, 1820, and 1821. By Brevet Major Gray. 8vo.

Journal of a Tour in Asia Minor. By W. Martin Leake, FRS. 8vo.

Lisbon in the Years 1821, 1822, and 1823. By Marianne Baillie. 2 vols. small 8vo.

JUST PUBLISHED.

Bostock's Elementary System of Physiology. 8vo. 15*s*.

The Zoological Journal, No. I. 10*s*. (To be continued Quarterly.)

The History of Ancient and Modern Wines. With Embellishments from the Antique. 4to. 2*l*. 2*s*. With the Vignettes on India paper, 3*l*. 3*s*.

Selection from De Humboldt, relating to the Climate, Productions, Mines, &c. of Mexico. By John Taylor, Treas. Geol. Soc. 8vo. 12*s*.

Travels in Brazil, in the Years 1817, 1818, 1819, and 1820, undertaken by the Command of the King of Bavaria. By Drs. J. Von Spix and C. Von Martius. Vols. I. and II. 8vo. 1*l*. 4*s*.

Critical Researches in Philology and Geography. 8vo. 8*s*.

The Perennial Calendar and Companion to the Almanac, as connected with History, Botany, Natural History, Astronomy, &c. &c. By Thomas Forster, FLS. &c. 8vo. 18*s*.

Naval Battles from 1744 to 1814, critically reviewed and illustrated. By C. Ekins, Rear-Adm, CB. &c. 4to. with 79 Plates and numerous Diagrams. 3*l*. 3*s*.

ARTICLE XVI.

NEW PATENTS.

J. Arrowsmith, Esq. Air-street, Piccadilly, for an improved mode of publicly exhibiting pictures or painted scenery of every description, and of distributing or directing the day-light upon or through them so as to produce many beautiful effects of light and shade, which he denominates Diorama.—Feb. 10.

R. Lloyd, Strand, hatter, and J. Rowbotham, of Great Surrey-street, Blackfriar's-road, Surrey, hat-manufacturer, for their having invented and brought to perfection a hat upon a new construction which will be of great public utility.—Feb. 19.

H. Adcock, Summer Hill Terrace, Birmingham, gilt toy manufacturer, for his improvement in making waistbands.—Feb. 19.

W. Church, Esq. Birmingham, Warwickshire, for certain improvements in machinery for printing.—Feb. 19.

A. Applegath, Duke-street, Stamford-street, Blackfriars, Surrey, printer, for certain improvements in machines for printing.—Feb. 19.

Rev. M. Isaacs, Hounsditch, for certain improvements in the construction of machinery, which, when kept in motion by any suitable power or weight, is applicable to obviate concussion by means of preventing counteraction, and by which the friction is converted into an useful power for propelling carriages on land, vessels on water, and giving motion to other machinery.—Feb. 19.

J. Vallance, Esq. Brighton, for his method of communicating goods or intelligence from one place to another with greater expedition than by means of steam-carriages, or other vessels.—Feb. 19.

A. H. Chambers, Esq. New Bond-street, Middlesex, for his invented improvements in preparing and paving horse and carriage ways.—Feb. 28.

R. Evans, Bread-street, Cheapside, wholesale coffee-dealer, for his process of roasting coffee and other vegetable substances, with improvements in the machinery employed.—Feb. 28.

J. Gunby, New Kent-road, Surry, sword and gun manufacturer, for a process by which a certain material is prepared, and rendered a suitable substitute for leather.—Feb. 28.

J. Christie, Mark-lane, merchant, and T. Harper, Tamworth, Stafford, merchant, for their improved method of combining and applying certain kinds of fuel.—Feb. 28.

W. Yetts, Great Yarmouth, merchant and ship-owner, for his invented certain apparatus to be applied to a windlass.—Feb. 28.

J. W. Richards, Caroline-street, Birmingham, metallic hot-house maker, for his improved metallic frame and lap, applicable to all hot-houses, green-houses, horticultural frames and glasses, sky lights, and other inclined lights and glasses.—Feb. 28.

W. Greaves, Sheffield, merchant, for certain improvements to harness, principally applicable to carriages drawn by one horse.—Feb. 28.

W. James, Westminster, land agent and engineer, for certain improvements in the construction of rail and tram roads or ways.—Feb. 28.

M. de Jough, Warrington, in the County Palatine of Lancaster, cotton-spinner, for his mode of constructing and placing a coke oven under or contiguous to steam or other boilers, so as to make the heat arising from making coal or other intense combustion in the said oven subservient to the use of the boiler, instead of fuel.—Feb. 28.

C. Bagenell Fleetwood, Gent. Parliament-street, Dublin, for his invented liquice and composition for making leather and other articles waterproof.—Feb. 28.

J. Spiller, Chelsea, Middlesex, engineer, for improvements in the machinery to be employed in the working of pumps.—March 6.

J. Heathcoat, Tiverton, Devon, lace manufacturer, for a new method of manufacturing certain parts of machines used in the manufacture of lace commonly called bobbin net.—March 9.

J. Heathcoat, Tiverton, Devon, lace-manufacturer, for his improved economical method of combining machinery used in the manufacture of lace in weaving and in spinning worked by power.—March 9.

W. D. Mosley, Radford, Nottingham, lace-manufacturer, for improvements in the making and working of machines used in the manufacture of lace commonly called bobbin net.—March 10.

W. Morley, Nottingham, lace-manufacturer, for various improvements in machinery now in use for the making lace or net commonly known by the name of bobbin net.—March 15.

R. Kirk, Osborne-place, Whitechapel, dyer, for his new method of manufacturing a certain vegetable substance, growing beyond the seas, as a dye or red colouring matter for the use of dyers called safflower, so as more effectually to preserve its colouring principle.—March 20.

ARTICLE XVII.

METEOROLOGICAL TABLE.

1824.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.
3d Mon.							
March 1	N W	29·94	29·82	45	28	—	02
2	N	29·82	29·12	38	27	—	
3	N W	30·02	29·12	34	27	—	05
4	N W	30·01	29·72	45	33	—	
5	W	29·97	29·72	50	39	—	—
6	S W	29·97	29·70	52	44	·44	07
7	S W	29·70	29·35	52	44	—	48
8	S W	29·87	29·35	52	34	—	09
9	S W	29·87	29·82	48	35	—	—
10	N E	30·11	29·83	45	28	—	03
11	W	30·16	29·71	48	30	—	28
12	N W	29·71	29·52	49	34	—	16
13	N W	30·00	29·51	48	33	—	03
14	N	30·20	30·00	46	27	—	
15	S	30·20	30·10	48	36	·46	06
16	W	30·26	30·10	48	40	—	
17	W	30·35	30·26	48	33	—	
18	N W	30·38	30·35	58	38	—	
19	N W	30·39	30·35	58	30	—	
20	E	30·39	30·05	55	41	—	
21	S W	30·05	29·78	48	36	—	23
22	Var.	29·93	29·76	43	29	—	34
23	N	30·08	29·93	48	35	—	07
24	N E	30·21	30·08	44	38	—	
25	N E	30·21	30·13	45	37	—	
26	N E	30·13	30·01	45	34	—	
27	N	30·03	29·91	44	32	·46	05
28	N E	30·12	30·03	43	23	—	01
29	N W	30·12	29·90	47	34	—	
30	N W	29·97	29·90	43	27	—	13
31	N	30·03	29·90	36	24	·25	
		30·39	29·12	58	23	1·61	2·05

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Third Month.—1. Fine morning: afternoon cloudy: evening rainy. 2. Fine. 3. Stormy, with snow, sleet, and rain, at intervals; and some hail. 4. Fine. 5. Showers. 6. Cloudy. 7. Rainy. 8. Morning rainy, with boisterous wind: cloudy. 9. Fine. 10. Cloudy and showery. 11. Rainy. 12. Stormy: showers of hail, rain, and sleet, during the afternoon. 13. Hail showers: sleet: driving wind. 14. Cloudy: windy. 15. Cloudy. 16—20. Fine. 21. Rainy. 22. Rain and sleet. 23. A considerable fall of snow this morning, in very large flakes: the day was afterwards fine. 24. Overcast: bleak. 25. Fine: cold. 26. Fair: bleak. 27. Showers. 28. Showers of snow and hail, with occasional gleams of sunshine. 29. Fine. 30. Fine: bleak: snow and hail showers. 31. Fine: bleak: a little snow.

RESULTS.

Winds: N, 5; E, 1; S, 1; SW, 5; W, 4; NW, 9; NE, 5; Var. 1.

Barometer: Mean height

For the month.................................. 29·958 inches.

For the lunar period, ending the 21st.................. 29·945

For 15 days, ending the 1st (moon south)........... 29·871

For 13 days, ending the 14th (moon north)........... 29·765

For 14 days, ending the 28th (moon south)........... 30·127

Thermometer: Mean height

For the month.................................. 40·048°

For the lunar period.................................. 40·133

For 29 days, the sun in Pisces.................... 39·844

Evaporation.. 1·61 in.

Rain.. 2·05

Laboratory, Stratford, Fourth Month 22, 1824. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

JUNE, 1824.

ARTICLE I.

Remarks on Solar Light and Heat. By Baden Powell, MA. of Oriel College, Oxford.

(*Continued from* p. 328.)

(18.) In attempting an inquiry into the constitution of the solar rays in reference to their heating power, I made the simple experiment described in my former communication, under an impression that every step in such an inquiry ought to be taken with the utmost caution; and that no position however probable ought to be assumed till sufficiently examined by experiment. Those experiments appear to me to prove that in the solar rays no free uncombined radiant heat exists, at least in any quantity sufficient to produce a rise of a quarter of a centigrade degree, on a thermometer coated with a wash of chalk. The same point, however, may be put to a more accurate and delicate test by means of Leslie's differential thermometer—an effect corresponding to the twentieth part of a centigrade degree may be thus rendered sensible.—(See Leslie on Heat, p. 11 and 420, and Treatise on Instruments, p. 10.)

Being particularly desirous of ascertaining whether it were possible to detect the smallest appreciable degree of simple radiant heat in the natural state of the solar rays, I continued the examination of the point by the following application of the differential thermometer. The instrument employed was of the "stationary" kind, the sentient ball was blown of black glass, and the halves of degrees could be very readily observed on its scale.

The surfaces of both balls being alike vitreous, they would be equally affected by any simple heat. If, therefore, a glass screen were so placed as to intercept the heat coming to one, but not that impinging on the other, the difference exhibited between this and the ordinary state of the instrument would make a very minute quantity of simple heat conspicuous. If any heat were thus intercepted on the interposition of the glass, the liquor would obviously rise towards the plain bulb. This it might do from the cooling effect of the glass; but if we observe first *with the glass* (it having remained sometime with the instrument), the effect would be perceived without this ambiguity on removing it.

Another photometer of the "portable" kind had the upper bulb coated with indian ink, and the lower washed with chalk. With this similar experiments were repeated. I give the following out of many which were all similar in their results.

(19.) Large photometer. Bulbs, black and plain glass. Graduation *from* the *black* bulb.

	Both exposed.	Glass over plain bulb two inches distance.
9·30 a. m.	69°	70°
	60	60
	68	68
2 p. m.	87	87·5
	88	88

Advantage was taken of moments when the indication of the instrument appeared tolerably stationary, which but rarely happens when it is used without its glass case. It here appears that this instrument could not detect any sensible degree of heat intercepted.

(20.) Small photometer. Upper bulb, indian ink. Lower bulb, washed over with chalk. Graduation *from* the upper bulb.

	Glass over lower bulb, half inch distance.	Both bulbs exposed.
(1.)	12°	15°
	17°	14° 18° 10°
	11° 14° 11°	13°
	20°	23° 20°
(2.)	20° 22°	23° 24°
	21° 22° 21°	22° 23°
	23° 18°	18° 19°

Here the fluctuations were more considerable than before; but on comparing all the results, it is obvious that the tendency is to an increase rather than a decrease when the glass was removed. This was probably owing to the glass acting in some measure as interceptive of the heat radiating *from* the whitened bulb, and thus altering the state of equilibrium in favour of the absorption of heat on its surface from the light.

These experiments tend to confirm the conclusion maintained in my former paper, and to extend the limits within which it holds good. I do not attribute any other importance to it than as contributing to lay a foundation of distinctness of ideas upon which to proceed in the further examination of the subject, and in comparing the radiant heating effect emitted from the sun with that from incandescent and burning bodies.

(21.) In reference to the validity of this opinion, however, we may make this further observation. Those rays of the sun which come within the reach of our examination have been here shewn to be entirely composed of one species characterised by the definition before laid down. It must, however, be admitted as by no means improbable, *that* the sun may originally give out a separate radiation of heat, distinguished by other properties, and of the same kind as the radiant heat from hot bodies. None of this kind *reaches us*, but we must consider the very different degree in which any medium, as air, absorbs or intercepts the passage of those two sorts of radiant matter. The *heat* from a hot body will not be perceptible at a short distance, while its *light* will traverse an amazing extent of length; and thus at different distances the ratio between the two will be very different. Some degree of simple heat, therefore, may actually be initially radiated by the sun, and be lost before it reaches us. We have no reason to believe that there is any medium between the different parts of the solar system capable of absorbing heat. The highest regions of our atmosphere into which observation has penetrated are uniformly the coldest; but they are known to have a greater capacity for heat. Thus though it is possible that some heat may reach to that distance and be absorbed without becoming sensible to us, its quantity must be very small: if, therefore, we suppose any simple heat to be initially radiated from the sun, it must be all or nearly all absorbed by some parts or appendages of that luminary exterior to the part where it is generated.

(22.) From considering the heating power which so inseparably accompanies the rays of light, which is always developed wherever they impinge on a surface which, from its colour, absorbs the rays, and which continues to act with very little diminution of intensity when the rays pass through transparent media of considerable thickness, we are led to observe some remarkable instances in which such effects are produced. Such an instance is afforded in the case of the eye, and the phenomena of vision.

What may be the immediate cause of vision, and what effects light may be capable of producing on the retina and optic nerve, we are altogether ignorant; but we may with tolerable certainty infer from well known facts and universal laws, that (among other effects) light must produce a *heating* effect on the black surface of the retina.

Every different shade of *colour* and every different *intensity* of light transmitted from objects, produces a *different* degree of heat on a black surface. It, therefore, follows, that when objects are painted on the retina, the rays coming from different parts of them communicate *different* degrees of *heat* by the absorption of its black coat, according to their different colour, brilliancy, &c. And since all our distinction of objects by the eye depends on their colour and reflecting power—on the differently coloured rays, and the total intensity of rays which they reflect, it is at least certain that the *perception* of objects, and their different parts, must be *accompanied* by corresponding perceptions of a difference of *heating* effect, whatever other distinctive impressions the different rays may be capable of producing on the sentient substance. I am far from meaning to assert the opinion that this is the immediate cause of vision. These observations are merely proposed as affording a curious topic of physiological inquiry.

The heating effect of light is produced at the moment of absorption, and is probably of a different nature from the communication of heat either by contact or by radiation. If, therefore, a non-luminous body could radiate different degrees of heat from its different parts so as to impinge on the retina, it would not produce the phenomenon of vision; but this state of circumstances cannot take place, since the transparent parts of the eye are not permeable to simple radiant heat; and if they became heated themselves, there would not be any distinction of *different* degrees of heat communicated to different parts of the retina. The optic nerve is the only one in the body expanded with an absorbing surface so as to be exposed to the external influence of radiant heating agents.

(23.) The consideration of the *heating* effects of light to the phenomena of vision must be attended to in the attempt to compare the illuminating with the heating power of light.

From the experiments of Sir W. Herschel, it appears that the greatest *illuminating* effect belongs to the yellowish-green rays. According to his theory, the radiant heat is separate from the light, and is formed of particles having less momentum, those of the green rays having such a momentum as is *best suited to the eye* for the purposes of vision, either *too little* or *too great* a momentum (as in the violet and red rays) being equally ill adapted for producing a vivid impression on the sight.

Independently of all hypothesis, the above remarks are of a nature deserving great attention in the consideration of these phenomena. The illuminating power of light must be referred not merely to any inherent properties of the rays, but to the nature of our organs. Hence, according to an observation just made, it must be inferred that either *too little* or *too great* an absorption of *light*, and consequent *heating* effect, on the pigmentum nigrum, is equally unfavourable to distinctness of vision, and the reason of this distinction may depend entirely on the peculiarities of the optic nerve in regard to its susceptibility to heat.

(24.) In the experiment alluded to by Newton (Optics, book 1, part 2, prop. 8), near the prism, white light occupies the central part of the spectrum, which at greater distances disappears, and yellow and green rays fill the space. According to his explanation of this phenomenon, it follows that the illuminating effect of the different parts of the spectrum must vary in their proportions at different distances from the prism.

(25.) The experiments of Sir W. Herschel (Phil. Trans. 1800) on the relative powers of different substances for intercepting light and heat, may be brought forward by some as a proof of the separate existence of two such agents in the solar rays; but those experiments, as it will be evident on a more attentive examination, do not bear at all upon this point.

The powers of different sorts of glass, &c. to intercept the sun's *light*, were measured by a comparison of the distances at which equal illuminating effects were produced when an object was viewed through the substance under trial, and by direct vision. Their powers of intercepting *heat* were determined by the comparative effects of the full solar rays on a naked thermometer.

The surface of the bulb of a thermometer, even when not coated with any pigment, will still absorb a considerable portion of the rays of *light* impinging on it, and will consequently be affected by the inseparable heating power developed by the absorption of those rays; and the effect will be *different* according to the *colour* of those luminous rays which the glass transmits. Now the relations of the *colour* of the rays to *heating*, and to *illuminating* effect, are by the experiments of the same author proved to be extremely different; so that the difference which appears to exist in the power which the same substance has to transmit light and heat is by no means a sufficient ground for concluding the separate existence of two sets of rays.

(26.) The consideration of the different laws followed by the illuminating and heating powers of the differently coloured rays, might lead us to doubt the close connexion between *intensity of light* and of *heating* effect. Indeed this consideration must be attended to in the attempt to compare by means of their heating effect on Leslie's photometer, light from different sources. In these cases there is often a considerable difference in the prevalent *colour* of the rays, and, therefore, from this source alone, some difference in their heating power is to be expected.

In relation to the different intensities of light from the same source, experiment seems to have shown that the closest proportion is observed. Prof. Leslie has compared the indications of his photometer with the measurements obtained by Bouguer from a comparison of illuminating effects, and conceives the result completely favourable to the accuracy of his instrument. (See Leslie on Heat, Chap. 20.)

With respect to the theory deduced from these and kindred phenomena by that distinguished philosopher, it cannot but excite admiration for its simplicity, and the readiness with which it explains the phenomena; but it surely cannot be considered as proved, that because light has the power of exciting or communicating heat in those bodies on which it impinges, that the light is, therefore, *transformed into heat*; and after all, the mode in which this transformation is effected in the "igneous fluid" still remains to be accounted for.

Towards ascertaining the nature of the heating power thus excited in the solar rays, one important step seems to me to be involved in the question whether the heating power does always, and especially at high intensities, increase precisely in proportion to the intensity of light, or number of rays impinging upon, or absorbed by, a given surface.

The want of a photometer on the principle of illumination, and that essential difficulty, the want of a standard for comparison, are great obstacles in the prosecution of such an inquiry. I have, however, attempted it by adopting other means in which these difficulties seem to me to be got rid of. As, however, the detail of these experiments will occupy some space, I must reserve them for a future opportunity.

ARTICLE II.

Astronomical Observations, 1824.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

April 14.	Immersion of Jupiter's third satellite	9h	19′	30″	Mean Time at Bushey.
		9	20	51	Mean Time at Greenwich.
April 30.*	Emersion of Jupiter's first satellite	10	25	00	Mean Time at Bushey.
		10	26	21	Mean Time at Greenwich.

* Jupiter's limb very tremulous.

ARTICLE III.

Remarks upon Dr. Berger's Reply. By Prof. Henslow.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Cambridge, May 5, 1824.*

I FELT somewhat surprised yesterday in perusing an angry article in your number of the *Annals of Philosophy* just published (for May), entitled, "Reply to Mr. Henslow's Observations on Dr. Berger's Account of the Isle of Man."

The examination which I made (in 1819) of the geological features of that island, was undertaken with a view of deriving instruction, rather than with any intention of giving it; as the study of geology was then almost new to me. I, therefore, procured Dr. Berger's paper; and here acknowledge myself indebted to it for almost the first rudiments of a science, which has since afforded me so much gratification. As I had observed a few omissions in that gentleman's description, as well as some errors with respect to the extent of certain formations laid down in his map, I presented my account to the Geological Society (together with a collection of specimens to which my remarks might be referred), by way of *supplement* to Dr. B.'s paper. From the "Reply," it should seem that I had been *misinformed*, in supposing his account was not to be viewed as so perfect a performance as he now professes to have considered it himself. It was under this impression that I troubled the Society with my remarks. I might, perhaps, have written to Dr. Berger (as he is pleased to suggest) had I been acquainted with his address; but I verily believed him to be no longer in existence.

So far as I can collect from Dr. B.'s "Reply," his observations, accusations, and concessions, may be reduced to the eleven following heads. Upon the fourth, fifth, eighth, and tenth of these, I shall offer a few remarks; the others appear unworthy of any comment. The charges, then, here brought against me, I consider to be,

1. The *correction of an error* in the geographical position assigned to the mountains of the south part of the island.
2. The *mention of a granitic district* unnoticed by Dr. Berger.
3. The *extension of the limits* assigned to a small patch of granite noticed in his paper.
4. The *union of the slates* under one common formation, instead of retaining them under the boundaries assigned by Dr. Berger to clayslate and greywacke.

To this I may observe, that the distinctions of mineral character, to which he refers, have long since been regarded (in this country at least) as an insufficient guide to a difference of epoch in the formations of clayslate, and the infinite varieties of greywacke. Unless, therefore, a distinction can be established from geological position, there can be no reason for separating them. It is possible such a distinction may exist in the Isle of Man, and I am even disposed to think it does, though the limits assigned by Dr. Berger appeared to me so very unsatisfactory that I preferred designating the whole on the map by an uniform colour. The propriety of this method has been confirmed by the learned and experienced geologist Dr. Macculloch, who has adopted precisely the same plan, in his account of this island, in the second volume of his History of the Western Isles of Scotland, published after my paper was transmitted to the Society, but before it had been printed.

Should Dr. Berger wish for further proof that he ought not to consider himself infallible on points of geology, I beg leave to refer him to Prof. Sedgwick's paper on the Geology of Cornwall, published in the Transactions of the Cambridge Philosophical Society.*

5. *Indistinctness* with regard to my "quartzose districts."

To this I am desirous of pleading guilty. I now feel but little doubt that these districts are merely subordinate to the clay slates. Want of experience, at the time I examined them, led me to suppose (from the marked difference of external character) that they might belong to a separate formation.

6. The *correction of the line* of boundary assigned by Dr. B. to the mountain limestone, or I should rather say *boundaries*, for

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* I may refer more particularly to the Professor's remarks in p. 135, but shall content myself with a few extracts from pp. 139, 140, and 141, as they happen to be directly applicable to the case in point.

"The rocks hitherto described in this section are considered by Dr. Berger as belonging to the greywacke formation; which, according to that author, extends, almost without interruption, on both sides of the central chain. To prevent any ambiguity, he first defines greywacke, &c. and he then divides the formation into common greywacke and greywacke slate. He afterwards adds, that in Cornwall, common greywacke is always found higher than greywacke slate, &c.—that it rests immediately upon the granite—that it is much less rich in ores than greywacke slate, &c. He then states,—that the greywacke slate becomes more perfectly schistose as it is farther removed from the granite—that its base is fine, smooth, and nearly homogeneous—that it sometimes possesses the lustre of satin—that it is to this second variety exclusively that the Cornish miners give the name of killas, &c.

"We cannot help considering the whole of this account *as inadequate*, and in a great measure *inapplicable*. No one term is sufficient to characterize the various beds of this formation, &c. If we assume with Dr. Berger, that common greywacke rests immediately on the granite; still *it is not true* that it is always found higher than the greywacke slate, &c. Again, if the common greywacke be much less rich in ores than the greywacke slate, then the part of the killas which rests immediately on the granite must be much less metalliferous than some other parts of the formation, and more remote from the fundamental rock. As a general observation, we believe this to be *untrue*, &c. But this is not all; we will venture to assert that the rock in immediate contact with the granite (at least in every instance in which we have ourselves examined it) bears *no resemblance whatever* to common greywacke. With *equal impropriety* we conceive the term greywacke slate applied to all the finer schistose beds of silvery lustre, which abound so much in this formation: because, &c. Lastly, *it is not true*, that the Cornish miners apply the term killas exclusively to these finer schistose beds, &c. &c."

there are two distinct localities for this formation in my account, though Dr. Berger has united them in one continuous tract on his map.

7. The *limitation of the old red sandstone* to its true extension, the western shore of Langness Point, instead of extending it (as Dr. B. had done) over the whole of that isthmus.

8. The *having mistaken him*, in stating the whole space between Scarlet and Poolvash to be occupied by a bed of amygdaloidal trap.

My mistake then amounts to this, that I had supposed Dr. B. to have been more accurate in his observations than he now allows himself to have been; for he mentions only the extreme points of a tract which is in fact wholly composed of trap rocks, although not of the peculiar variety he alludes to. A juster cause for observation might have been found in our difference of opinion with respect to the *position* of this trap, which he states to exist as an overlying mass, but which I am bold enough to contradict, because I think the evidence is clearly in favour of its intruding from below.

9. The *assertion* that the term Curragh is sometimes applied to mountain peat-bogs, and does not exclusively belong to the flat district towards the north of the island.

N. B. Although Dr. B. is pleased to ridicule the idea of correcting a marked *geographical* error in a geological map, we find him expatiating with complacency upon the utility of his observations with regard to *husbandry*.

10. The *mention of the discovery* of a number of elks' bones, and earthy phosphate of iron, in the marl, near Kirk-Balaff.

Dr. Berger insinuates that I consider this circumstance as *extraordinary*. Upon a careful perusal of the passage, I cannot perceive the slightest evidence of any such intimation. The number of bones imbedded in the small extent of the marl is somewhat extraordinary, and the fact of an entire skeleton being discovered was new. That the circumstance itself was not of a novel description I was perfectly aware, and introduced the subject by stating, that here "are found the remains of *the* gigantic elk."

11. The *want of "clear-sightedness"* in not being able to comprehend the latent meaning of the following sentence: "Some go so far as to say, that the increase is no less than two yards in a year." Dr. B. assures us that this sentence *evidently* implies, that he *discredited* the information it apparently conveys.

I shall now conclude by observing that Dr. Berger's "Reply" to my "prolix and inflated criticism," and the courteous hint, that it was dictated by "the malignity of envy," have caused me no other sensation than regret—for the loss of a morning in penning these remarks.

I have the honour to be, Sir, your obedient servant,

J. S. Henslow.

Article IV.

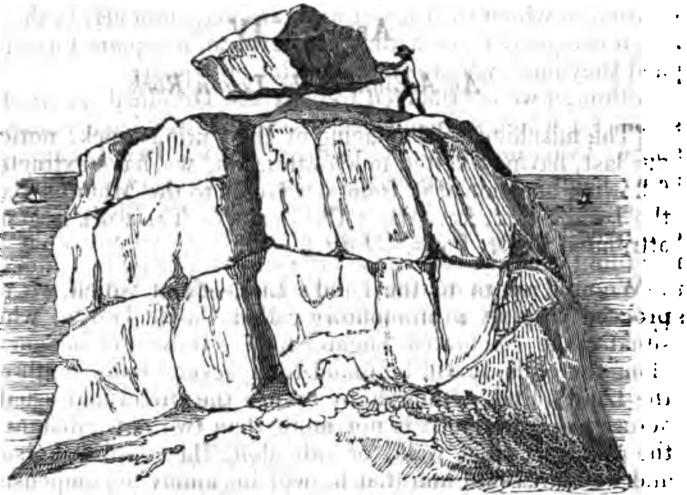
An Account of the Logan Rock.

[The mischievous displacing of this curious rock, noticed in our last, having excited much attention, we have extracted the following account of it from a "Guide to the Mount's Bay and the Land's End, &c. By a Physician." The work is generally attributed to Dr. Paris.—*Edit.*]

We now return to the Land's End,—from which we should proceed to visit a promontory called Castle Treryn, where is situated the celebrated Logan Stone. If we pursue our route along the cliffs, it will be found to lie several miles south-east of the Land's End, although by taking the direct and usual road across the country, it is not more than two miles distant; but the geologist must walk, or ride along the coast on horseback, and we can assure him that he will be amply recompensed for his trouble.

From the Cape on which the signal station is situated, the rock scenery is particularly magnificent, exhibiting an admirable specimen of the manner and forms into which granite disintegrates. About forty yards from this Cape is the promontory called Tol-Pedn-Penwith, which in the Cornish language signifies the holed headland in Penwith. The name is derived from a singular chasm, known by the appellation of the Funnel Rock; it is a vast perpendicular excavation in the granite, resembling in figure an inverted cone, and has been evidently produced by the gradual decomposition of one of those vertical veins with which this part of the coast is so frequently intersected. By a circuitous route you may descend to the bottom of the cavern, into which the sea flows at high water. Here the Cornish Chough (*Corvus Graculus*) has built its nest for several years; a bird which is very common about the rocky parts of this coast, and may be distinguished by its red legs and bill, and the violaceous blackness of its feathers. This promontory forms the western extremity of the Mount's Bay. The antiquary will discover in this spot the vestiges of one of the ancient Cliff Castles, which were little else than stone walls, stretching across necks of land from cliff to cliff. The only geological phenomenon worthy of particular notice is a large and beautiful contemporaneous vein of red granite containing shorl; is one foot in width, and may be seen for about forty feet in length.

Continuing our route around the coast we at length arrive at Castle Treryn. Its name is derived from the supposition of its having been the site of an ancient British fortress, of which there are still some obscure traces, although the wild and rugged appearance of the rocks indicate nothing like art.



The foundation of the whole is a stupendous group of granite rocks, which rise in pyramidal clusters to a prodigious altitude, and overhang the sea. On one of those pyramids is situated the celebrated Logan Stone, which is an immense block of granite weighing above 60 tons. The surface in contact with the under rock is of very small extent; and the whole mass is so nicely balanced, that, notwithstanding its magnitude, the strength of a single man applied to its under edge is sufficient to change its centre of gravity, and though at first in a degree scarcely perceptible, yet the repetition of such impulses, at each return of the stone, produces at length a very sensible oscillation! As soon as the astonishment which this phenomenon excites has in some measure subsided, the stranger anxiously inquires how, and whence the stone originated—was it elevated by human means, or was it produced by the agency of natural causes?—Those who are in the habit of viewing mountain masses with geological eyes, will readily discover that the only chisel ever employed has been the tooth of time—the only artist engaged, the elements. Granite usually disintegrates into rhomboidal and tabular masses, which by the farther operation of air and moisture gradually lose their solid angles, and approach the spheroidal form. De Luc observed in the Giant mountains of Silesia, spheroids of this description so piled upon each other as to resemble Dutch cheeses; and appearances, no less illustrative of the phenomenon, may be seen from the signal station to which we have just alluded. The fact of the upper part of the cliff being more exposed to atmospheric agency, than the parts beneath, will sufficiently explain why these rounded masses so frequently rest on blocks which still preserve the tabular form;

and since such spheroidal blocks must obviously rest in that position in which their lesser axes are perpendicular to the horizon, it is equally evident, that whenever an adequate force is applied they must vibrate on their point of support.

Although we are thus led to deny the Druidical *origin* of this stone, for which so many zealous antiquaries have contended, still we by no means intend to deny that the Druids employed it as an engine of superstition; it is indeed very probable that, having observed so uncommon a property, they dexterously contrived to make it answer the purposes of an ordeal, and by regarding it as the *touchstone* of truth, acquitted or condemned the accused by its motions. Mason poetically alludes to this supposed property in the following lines:

> "Behold yon huge
> And unknown sphere of living adamant,
> Which, pois'd by magic, rests its central weight
> On yonder pointed rock: firm as it seems,
> Such is its strange, and virtuous property,
> It moves obsequious to the gentlest touch
> Of him, whose heart is pure, but to a traitor,
> Tho' e'en a giant's prowess nerv'd his arm,
> It stands as fix'd as Snowdon."

The rocks are covered with a species of Byssus long and rough to the touch, forming a kind of hoary beard; in many places they are deeply furrowed, carrying with them a singular air of antiquity, which combines with the whole of the romantic scenery to awaken in the minds of the poet and enthusiast the recollection of the Druidical ages. The Botanist will observe the common Thrift (*Statice Armeria*) imparting a glowing tinge to the scanty vegetation of the spot, and by growing within the crevices of the rocks, affording a very picturesque contrast to their massive fabric. Here too the *Daucus Maritimus*, or wild carrot; *Sedum Telephium*, *Saxifraga Stellaris*, and *Asplenium Marinum*, may be found in abundance.

The granite in this spot is extremely beautiful, on account of its porphyritic appearance; the crystals of felspar are numerous and distinct; in some places the rock is traversed by veins of red felspar, and of black tourmaline, or schorl, of which the crystalline forms of the prisms, on account of their close aggregation, are very indistinct. Here may also be observed a contemporaneous vein of *schorl rock* in the granite, nearly two feet wide, highly inclined and very short, and not having any distinct walls. On the western side of the Logan rock is a cavern, formed by the decomposition of a vein of granite, the felspar of which assumes a brilliant flesh-red, and lilac colour; and where it is polished by the sea, exceeding even in beauty the *Serpentine caverns at the Lizard.*

Mr. Majendie observed in this spot numerous veins of fine-grained granite, which he is inclined to consider as *cotemporaneous*; he also observed what, at first sight, appeared to be fragments, but which, upon closer examination, he pronounced to be *cotemporaneous concretions*; for large crystals of *felspar* may be seen shooting from the porphyritic granite into these apparent fragments. These phenomena are extremely interesting in a geological point of view, and well deserve the attention of the scientific tourist.

ARTICLE V.

Analysis of the Fulminate of Silver. By MM. Liebig and Gay-Lussac.*

THE memoir published by one of us upon fulminating silver and mercury, prepared by means of nitric acid and alcohol, was principally intended to show that these compounds are true salts formed by peculiar acids which may be separated from them, and combined with all other bases. Their analysis, and especially those of the acids, opposed too many difficulties to allow of the flattering conclusion, that these were given with accuracy on this first essay; and being both of us persuaded of the possibility of effecting them with a greater degree of precision, we united to make this analysis the subject of fresh researches.

Fulminate of silver being very easily prepared, and its insolubility allowing of its being had perfectly pure, we selected it in preference to other fulminates to submit it to experiment.

The process by which we prepared it differed but little from those which have been given; nevertheless it occurred to us that it might not be useless to state the manipulations of it.

Put into a pint matrass 700 grs. of nitric acid, of specific gravity 1·36, or 1·38, and 34 grs. of pure silver. When the silver is dissolved, the solution is to be poured into 1000 grains of alcohol of 85 to 87 centisimal degrees. The fluid when heated to ebullition soon becomes turbid, and begins to deposit fulminate of silver; the matrass is then to be removed from the fire, and in order to diminish the ebullition, which still continues, a quantity of alcohol equal to that originally employed is to be gradually added. When the ebullition has ceased, it is to be suffered to cool, the fulminate is to be put upon a filter, and washed till it ceases to yield any acid. The fulminate is then snow-white and quite pure. The filter is then to be removed, and spread upon a plate, and covered with paper; it is to be placed upon a vessel half filled with water, which is to be kept boiling for two or three hours. The weight of fulminate obtained is usually equal to that of the silver employed; a third more ought to be procured; but this third remains in solution in the nitric acid, and in the washings.

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* Annales de Chimie et de Physique, tome xxv. p. 285.

Fulminate of silver does not detonate alone at a temperature of 212° Fahr., nor even at 266°; but it must not be exposed to the slightest percussion between two hard bodies, even when it is in water. Consequently wooden stirrers only should be employed, instead of glass ones, and the capsules on which it is put should be placed upon several folds of paper; it is prudent also to remove it only with bits of paper; for the detonation of a few grains only of this matter occurring on the hand would infallibly occasion its loss.

We ascertained, by operating upon very small quantities of fulminate, that it might be rubbed on a porcelain capsule with a cork or the finger, after having mixed it with forty times its weight of peroxide of copper, and that it did not detonate when exposed to the action of heat; we employed this method of determining what were the proportions of carbon and azote in the fulminate of silver. Two decigrammes (grs. 3·0888) of this salt mixed with four times its weight of oxide of copper, and heated in a glass tube produced a gaseous mixture, the last portions of which, after the expulsion of the common air, were composed of two parts by volume of carbonic acid gas and one volume of azote; consequently in the fulminate of silver, or rather in the fulminic acid, the carbon and the azote are in the same proportions as in cyanogen.

Fulminate of silver containing two portions of oxide of silver, one of which serves as a base to the salt, and the other appears to be a constituent of fulminic acid, we endeavoured to determine each with precision. Their total quantity is easily obtained by decomposing fulminate of silver by muriatic acid, and evaporation to dryness; towards the end of the operation, a little nitric acid is to be added in order to decompose a small quantity of muriate of ammonia, which is formed during the evaporation, and which is derived from the decomposition of an acid, which we shall presently mention.

2·266 parts of fulminate of silver, thus decomposed, yielded 2·171 of chloride of silver. Reducing this chloride to oxide of silver, 100 of fulminate contain 77·511 of oxide of silver; in a second experiment 1·060 of fulminate yielded 1·016 of chloride of silver, or 100 of fulminate contain 77·546 of oxide of silver.

By taking the mean of these two experiments, 100 of fulminate of silver contain 77·528 of oxide of silver, or

Silver	72·187
Oxygen	5·341
	77·528

We suppose that all the silver is in the state of oxide, and it will be seen that this supposition is extremely probable.

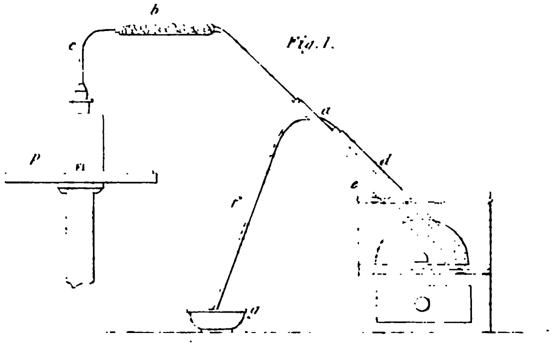


Fig. 1.

Fig. 4.

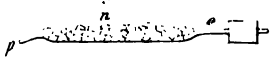


Fig. 2.

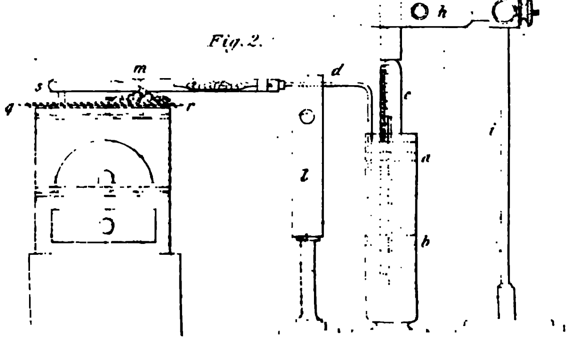


Fig. 3.

Fig. 5.

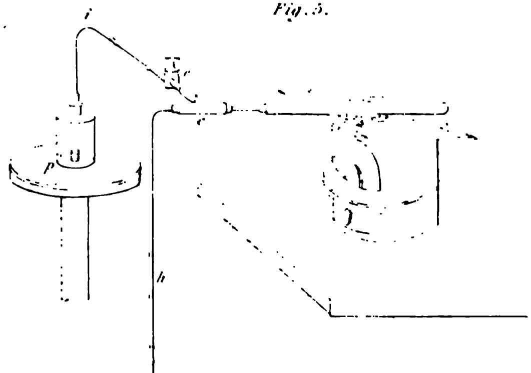
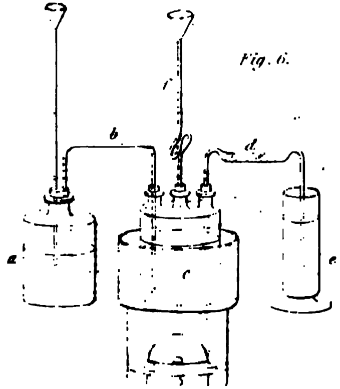


Fig. 6.



When fulminate of silver is put into a solution of potash, oxide of silver is separated, and fulminate of potash is formed; but the decomposition is very incomplete; oxide of silver continues to precipitate during the evaporation of the fluid, even at the expiration of some days; the results obtained are very variable, depending upon the quantity of potash, and unquestionably also upon the formation of double compounds.

One hundred parts of fulminate of silver produced 27·14; 29·69 and 31·45 of oxide of silver. Being unable to employ this method of analysis to determine the quantity of base combined with the fulminic acid, we had recourse to chloride of potassium; it does not indeed decompose the fulminic acid; it precipitates the oxide of silver combined with it in the state of chloride, and produces fulminate of potash; 2·252 parts of fulminate of silver thus decomposed by chloride of potassium slightly in excess, yielded 1·202 of chloride of silver; estimating this quantity of chloride in the state of oxide, 100 of fulminate contain 38·106 of oxide of silver as a base.

The fulminate of potash obtained in this experiment, decomposed by muriatic acid, yielded 1·210 of chloride of silver; or 100 of fulminate contain 38·359 of oxide of silver regarded as a constituent of the fulminic acid. These two quantities of oxide of silver differ so little from each other that we may conclude the fulminate of silver to contain twice as much oxide as is required to saturate the fulminic acid. The total of these two quantities of oxide amounts only to 76·464, instead of 77·528, which ought to have been obtained; but the conclusion which we have drawn from our experiment is not on this account much less accurate.*

Knowing the quantity of oxide contained in the fulminate of silver, we endeavoured to determine its other elements, among which are, as we already know, carbon and azote. We decomposed the fulminate of silver by oxide of copper; but as it was very requisite to dry the matters upon which we had to operate perfectly, we shall begin by describing the process by means of which we believe that we succeeded in effecting it; especially as it is applicable to the analysis of any vegetable or animal substance.

After having mixed the fulminate of silver with the oxide of copper, and introduced the mixture into a moderately thick glass tube, the external diameter of which was about one-third of an inch, and its length nearly twelve inches, *a*, fig. 1 (Pl. XXVIII), it was connected with a tube *b*, containing chloride of calcium, which is itself adapted, by means of a flexible lead pipe, *c*, to a small receiver placed upon the plate *p* of an air-pump.

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* The alkaline chlorides possess the property of dissolving a small quantity of chloride of silver: this source of error we avoided in the following manner: we began by evaporation nearly to dryness, and poured some nitric acid upon the residuum: by heat, the chloride of potassium was quickly converted into nitrate, while the chloride of silver suffered no change.

On making a vacuum in the apparatus, the air carries with it the vapour of water, and re-enters the tube containing the mixture only when dried by the chloride of calcium. But in order still better to separate the hygrometric moisture of the mixture, the tube containing it is placed in a tube, *d e*, of greater diameter, fitted with a perforated cork, and filled with water which may be boiled. The vapour escapes by the tube, *f*, and the water which condenses in it falls into the vessel *g* placed below it. By alternately filling and exhausting the apparatus, it will be granted that the mixture must lose all its hygrometric moisture. With respect to other substances, the decomposition of which there is no reason to fear at a temperature above 212°, the tube containing the mixture may be heated in an acid or saline solution, or in a bath of oil. The apparatus which has been described requires no copper pipe; the joinings are all made with cork, and when this substance is of a good quality, the apparatus sustains a vacuum perfectly without the use of any cement, or, at most, a little isinglass or tallow may be introduced into the pores of the cork when it appears to have any.

The mixture of fulminate of silver and oxide of copper being perfectly dried, it is decomposed by the action of heat, and the gases resulting from this decomposition are to be received; but, as by the usual processes, it is difficult to ascertain their real volume, we employed the following apparatus which gives it immediately.

This consists of a footed receiver, *a b*, fig. 2, into which are cemented, one in *a*, and the other in *b*, two rings or collars of cork, for the purpose of directing the small graduated receiver, *c*, in its motion. The tube, *d*, which conducts the gases into the graduated receiver, has two vertical and parallel branches, the ascending one of which nearly touches the top of the graduated receiver when it is in its lowest place, and then passes on the outside of the graduated receiver between the two openings of the cork rings. The plan of these rings is represented by fig. 3. The footed receiver being filled with mercury, and the ascending branch of the conducting tube being placed in the graduated receiver, the latter is plunged in mercury, and the air escapes gradually by the conducting tube. The receiver is fixed in its first position, by supporting it from its summit with a cork fixed in a wooden arm, *h*, moving along a vertical stand, *i*, upon which it may be stopped at any situation by a screw, *k*. The tube, *m*, containing the mixture, is then adapted to the conducting tube, and the latter is confined in the wooden vice of the support, *l*, the parts of which are made to approach by means of a screw, and separate by their own spring. The mercury in the graduated receiver is put exactly on a level with that which forms the bath, and the volume of air in the graduated receiver is noted, as well as its temperature. When the mixture is decomposed, the gases which are evolved depress the mercury in the graduated tube; but by properly shifting the wooden arm along its support, the mercury is maintained at nearly its original level, it being continually added to fill the space which the receiver leaves by rising out of the bath. When the decomposition is finished, the fire is removed, and when the apparatus is cooled, the mercury is brought to the same level in the receiver and in the bath, and the temperature is noted. It is evident that the volume of air contained in the graduated receiver, after the operation, less than that which existed there before it, indicates exactly the volume of the gases which result from the decomposition, the corrections for temperature and barometric pressure being made; but as the whole of the operation continues at most but half an hour, it is seldom requisite to make any corrections.

The water which is formed during the decomposition of an hydrogenous substance by the oxide of copper, is usually collected by causing it to pass over chloride of calcium contained in a tube placed between the conducting tube and that which contains the mixture; but the following arrangement, which consists in introducing the chloride of calcium into the same tube as that in which the decomposition is effected, has appeared to us to be preferable.

We take a very small tube, *n*, fig. 4, the external diameter of which is nearly equal to the internal diameter of the tube, *m*, containing the mixture; to this a small tube, *o*, is attached by melting them together, and it is made to pass tightly through the tube, *m*, and after having filled it with chloride of calcium, it is drawn to a point at the other end, *p*, leaving only a small aperture. Having noted the weight, it is to be placed in the tube, *m*, as shown by fig. 2; the gases have then no other passage for escape but over the chloride of calcium, and they then deposit their moisture. When the mixture is introduced into the tube, *m*, care must be taken to leave an empty space, *m s*, below the upper side of the tube, that it may not be projected forward by the gases at the moment of their expulsion. Lastly, it has been often recommended to use a spirit-lamp to effect the decomposition of the mixture; but we find it to be much more convenient to place the naked tube upon a grating of iron wire, supported by a furnace, the door and ash-hole of which are kept shut, and to surround it with red-hot charcoal occasionally added. This method also possesses the advantage of enabling us to heat the whole of the tube in every part at the same time, and with a little practice, it is easily rendered of a dull red heat, without any risk of softening it.

The process of analysis by oxide of copper being very well known, we shall limit ourselves to the relation of the results which we obtained, without going into further details.

We usually experimented upon three decigrammes (4·6332 grains) of fulminate of silver, and considering the carbon and azote disengaged as existing in the state of cyanogen, five successive experiments gave us:

Fulminate of silver 100; cyanogen	17·379
	17·315
	16·921
	16·869
	17·314
Mean	17·160

In the first experiment no trace of water was perceptible; in the second 0·0616 of a grain; in the third 0·0154 grain, in the fourth 0·1848 grain, and in the fifth 0·0308 of a grain was obtained.

Although in these several experiments, except in the fourth, the quantity of water is not considerable, we did not consider it as accidental until after we had ascertained in several ways that its quantity was never sufficient to allow hydrogen to be considered as one of the elements of fulminate of silver. We generally operated upon three decigrammes (4·6332 grains), of fulminate, and supposing the hydrogen which it contains to be in sufficient quantity to form hydrocyanic acid with the cyanogen, this quantity ought to have yielded 0·2710 grain of water, which certainly could not have escaped our notice.

We shall presently adduce other proofs of the absence of hydrogen on the fulminates, and in the mean time, we conclude that this compound is formed of

Silver	72·187
Oxygen	5·341
Cyanogen	17·160
Loss	5·312
	100·000

The quantity 5·312 is evidently equal to the quantity of oxygen combined with the silver; it cannot be attributed to the hydrogen, which, supposing there had been any combined with the cyanogen to form hydrocyanic acid, would amount only to 0·651; nor can this loss be attributed to water, for we have never found a quantity nearly equal to it; consequently it can be attributed only to oxygen contained in the fulminic acid. According to this supposition, which will be hereafter verified, fulminate of silver consists of

Two atoms of silver,

Two atoms of oxygen combined with the silver,

Two atoms of oxygen combined with the elements of the fulminic acid.

$$\text{Two atoms of cyanogen} = \begin{cases} \text{2 atoms of azote,} \\ \text{4 atoms of carbon.} \end{cases}$$

It evidently follows from this analysis, that fulminate of silver does not contain sufficient oxygen to convert its carbon into carbonic acid. The examination of the residuum left by the fulminate of silver after decomposition by oxide of copper, in which copper is found in its metallic state, gives an incontrovertible proof of this; but it would not be easy to determine by this method the quantity of oxygen really deficient.

It would have been important to have ascertained the products of the immediate detonation of the fulminate of silver; and we did not fail to make some experiments with this intention; but we were compelled to abandon it on account of the fracture of the vessels, which took place with a very small quantity of fulminate, and by the danger inseparable from experiments of this kind. On the other hand, it appeared very easy to determine the products of the decomposition of the fulminate of silver by heat, after having mixed it with substances incapable of supplying it with oxygen: that which at first appeared preferable to us was glass reduced to an impalpable powder; but every time that we endeavoured to mix it with the fulminate, detonation took place, and prudence compelled us to give up this method.

By employing chloride of potassium instead of glass, the mixture may be rubbed by the finger, or with a cork, without danger, after having mixed it as intimately as possible with a small slip of card.

A portion of fulminate of silver weighing 6·131 grains (0·397 gramme), decomposed by this process, produced 11·929 cubic inches of gas; with oxide of copper, this same quantity of fulminate would have given 36·81 cubic inches. The 11·929 cubic inches of gas contained no oxide of carbon, and were composed merely of azote and carbonic acid. Now, according to this composition, if all the oxygen of the fulminate had been employed to form carbonic acid, and the azote had been disengaged with it, and recollecting that in the complete combustion of the fulminate by oxide of copper, the azote constitutes one-third of the whole volume, and the carbonic acid two-thirds, there should have been obtained

Azote = $\frac{1}{3}$ of 36·81 = ... 12·27

Carbonic acid = $\frac{1}{3}$ of 36·81*....... 12·27

24·54

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* We say one-third of 36·81, because, in our experiment, only one-half the quantity of carbonic acid could be formed that would result from the complete combustion of the carbon.

There being so great a difference between the results of calculation and experiment, we determined to examine the residuum of the distillation, which was of a blackish-grey colour. We found by treating a portion of it with water, that it was very alkaline, and that which was not dissolved contained much chloride of silver. Thus by the agency of the silver, and the oxygen contained in the fulminate, a part of the chloride of potassium was decomposed, and converted into potash and chloride of silver, and the former was combined with carbonic acid: this circumstance explains sufficiently why so small a volume of gas was obtained.

During the decomposition of the fulminate of silver mixed with the chloride of potassium, a small quantity of carbonate of ammonia was obtained, which was collected in one part of the tube by surrounding it with a piece of paper moistened occasionally with ether to cool it. For the purpose of determining the quantity of carbonic acid combined with the ammonia, we passed up a tube over mercury some muriatic acid, and a small fragment of marble insufficient to neutralize it. We agitated it thoroughly, in order to favour the solution of the carbonic acid gas in the muriatic acid, and some time after we introduced into the solution the portion of the tube upon which the carbonate of ammonia had condensed: effervescence was very perceptible; but the gas disengaged was not equal to 0·09 of a cubic inch; consequently the quantity of ammonia contained in the carbonate could not amount to 0·18 of a cubic inch.

The residuum of the decomposition of the fulminate of silver necessarily containing some carbon, it was distilled with oxide of copper; 17·874 cubic inches of gas were obtained, which, added to the 11·929 before procured, give a total of about 29·8 cubic inches; the difference between this quantity and 36·81 which ought to have been obtained, is still very great; but the potash formed, necessarily retained a portion of carbonic acid, and on other accounts this experiment was not sufficiently correct.

The chloride of potassium not having answered our purpose, we employed sulphate of potash, calcined and reduced to a very fine powder, that it might be rubbed with fulminate of silver without danger. 5·32 grains of fulminate mixed with about twenty times as much sulphate of potash, and dried in a vacuum, yielded by distillation 14·68 cubic inches of gas; the same quantity of fulminate distilled with oxide of copper would have given 32·04 cubic inches. The residuum heated with oxide of copper afterwards gave 14·96 cubic inches of gas; but we remarked that it was rather red, and that consequently nitrous acid was formed: this happened unquestionably, because, in order that the bulk of the mixture might not be too great, too small a proportion of oxide of copper had been used. There was also produced a small quantity of carbonate of ammonia, which appeared to us to be smaller than that obtained in the preceding experiment, and no trace of water was perceptible, which seems to prove that the formation of one of these compounds prevented that of the other. Now, supposing that all the hydrogen combined with the cyanogen in the fulminate of silver was combined with the azote to form ammonia, a very appreciable quantity of carbonate of ammonia ought to be obtained.

Indeed the 5·82 grains of fulminate decomposed by oxide of copper ought to yield 32·04 cubic inches of gas, composed of two-thirds of carbonic acid and one-third of azote. And as there is in hydrocyanic acid as much hydrogen in volume as of azote, 32·04 cubic inches represent $\frac{32\cdot04}{3} = 10\cdot68$ of hydrogen gas, which, combined with their third of azote, ought to produce $\frac{10\cdot68 + 3\cdot56}{2} = 7\cdot12$ of ammoniacal gas: this quantity would absorb 3·56 of carbonic acid gas to form carbonate of ammonia, and the total diminution which would result from the disappearance of carbonic acid gas and that of azote, would be equal to 7·12 cubic inches. In our experiment, notwithstanding the formation of nitrous acid, we obtained 29·64 cubic inches, the difference between which and 32·04 is very far from being equal to that which ought to be obtained, if all the supposed hydrogen had been taken up to form ammonia. The hypothesis that a portion would produce water cannot be admitted; for, as we have already remarked, when carbonate of ammonia is formed, not the slightest trace of moisture is perceptible; and further, we have proved by direct experiment, that by moistening the fulminate of silver, much carbonate of ammonia is obtained.

Thus the decomposition of fulminate of silver mixed with sulphate of potash furnishes us with additional proof that it does not contain hydrogen as one of its elements. The fulminate of silver in the preceding experiment having given two portions of gas, one with sulphate of potash, and the other with oxide of copper, it was important to ascertain the nature of each of them. We made a fresh experiment directed solely to this end; but being desirous of obtaining the first portions evolved without any admixture of atmospheric air, we endeavoured to form a vacuum in our apparatus.

To the tube containing the mixture, we adapted a copper tube, *t*, fig. 5, connected with a glass tube, *d*, nearly 40 inches long, and immersed it in a basin of mercury, *m*, in order to collect the gases. From the middle of the copper tube, another, *c*, projects at a right angle, furnished with a cock, and communicating with the air-pump by means of a leaden pipe, *l*. On making a vacuum in the apparatus, the mercury cannot pass the height, *h*, equal to about thirty inches, and by then turning the cock, all communication between the apparatus and the air-pump is shut off.

In employing this apparatus, we found that the gas evolved during the distillation of the fulminate of silver with the sulphate of potash, is composed of two volumes of carbonic acid gas and one of azote, and that obtained in distilling the residuum with oxide of copper consists of 100 volumes of the first gas and 37·4 of the second.

Although this result is not in perfect agreement with the first, and although the experiments which we detail have not all the accuracy we could wish, it appears to us nevertheless probable, that in the decomposition of the fulminate of silver mixed with the sulphate of potash, only half of the carbon is converted into carbonic acid; that a quantity of azote is evolved which corresponds exactly with what ought to happen if the azote and the carbon were in the fulminate in the state of cyanogen, and that consequently the silver in the residuum exists in the state of subcyanuret.

If the elements, which analysis has made us acquainted with, in the fulminate of silver are the true ones, it is easy to obtain the true equivalent number of fulminic acid; for admitting that the oxide of silver which acts as a base in the fulminic acid is exactly half that contained in the fulminate, we have $\frac{77 \cdot 525}{2} =$ 38·764 : 61·236 :: 145·161 (oxide of silver) : 229·31 : now, by calculation, fulminic acid will be composed of

1 atom of oxide of silver	145·161
2 atoms of cyanogen	65·584
2 atoms of oxygen	20·000
	230·745

In order to verify this result, we prepared fulminate of barytes by decomposing fulminate of silver with chloride of barium, and after having dried it at the temperature of 212°, we treated it with muriatic acid, which formed chloride of barium and chloride of silver: 38·33 of fulminate of barytes produced 15·85 of chloride of barium, and from this the equivalent number of fulminic acid is readily deduced, and is 228·873. The agreement between these three results is as near as can be expected in experiments, the danger attending which does not allow of their repetition, and we shall admit 230·745 as the equivalent number of fulminic acid, which is the result of calculation.

Being now acquainted with the nature of the elements of fulminate of silver, we shall direct our attention to the manner in which they are combined.

If silver be an essential principle of fulminic acid, we must necessarily admit the existence of almost as many peculiar acids as there are metals; for the greater part may replace silver, and form each a fulminic acid. With zinc alone for instance, a fulminate is produced which is perfectly analogous to that of silver; but is it probable that bodies, the properties of which are so different, should replace each other in the same atomic proportion, and form, with cyanogen and oxygen, acids which are perfectly similar? or is it not, on the contrary, more probable that the various fulminic acids are real supersalts, the acid of which does not contain any metal as one of its elements, and which is formed only of oxygen and cyanogen? It must be confessed that our experiments render this opinion extremely probable; but the following considerations give it additional certainty.

As fulminates may be obtained without silver or mercury, with oxides which yield oxygen with difficulty, as, for example, the oxide of zinc, it necessarily follows that the various fulminates contain a common principle of fulmination, which is independent of their bases, and which can be only a compound of oxygen and cyanogen, or, if it be preferred, a compound of oxygen, carbon, and azote.

In addition to this, if we compare the fulminates with the neutral tartrates, and the various fulminic acids to the several bitartrates, perfect analogies exist between them. Thus a neutral tartrate of zinc, copper, silver, or mercury, &c. is only partially decomposed by potash, in the same manner as the fulminates of these bases; all the fulminic acids form double salts with bases like the bitartrates; fulminic acid with silver as a base is precipitated by acids on account of its insolubility in the same circumstances as cream of tartar; and there are many fulminates, like the neutral tartrates, in which acids produce no precipitate, because the corresponding acidulous fulminates or tartrates are soluble; such are the fulminates and tartrates of zinc and copper. Lastly, the fulminates have great analogy with the hyposulphites.

According to these analogies, it appears to us extremely probable, not to say certain, that the several fulminates form a particular kind of salts, all containing the same acid, composed only of an atom of cyanogen and an atom of oxygen, and which is unquestionably *cyanic acid*. The neutral fulminates would be *cyanates*, the various fulminic acids *bicyanates*, and the equivalent number of cyanic acid would be 42·792, that of oxygen being equal to 10. Nevertheless in proposing the name of *cyanic acid*, which appears to express the nature of the fulminating principle common to all the fulminates, we request, before it is adopted, that our results should be verified by chemists; and on this account, we shall continue to employ, with the new acceptation determined by our experiments, the names of *fulminic acid* and *fulminates*, which not indicating the nature of the compounds to which they are applied, will have the provisional advantage of not inducing any error.

In recollecting the property of the *amer* described by Welter, of forming detonating salts with bases, we could not avoid looking for some analogy between this compound and the acid of the fulminates, although, on other accounts, we were fully persuaded that their nature is not similar. The analysis of the salts formed with amer was the only means of clearing up this subject; but having been able to give but little time to the preparation of this substance, and not having succeeded in attaining a quantity sufficiently pure, we have been compelled to defer this analysis to a future period.

The nature of the fulminates appearing to be determined, we made some attempts to separate the fulminic acid from them; but they were all unsuccessful: for either the fulminates are not decomposed by the acids, or, when they are, the fulminic acid is so also, and yields peculiar products, respecting which we are going to offer some observations, although they are incomplete.

The muriatic, hydriodic, and hydrosulphuric acid, decompose fulminate of silver, even when cold; with muriatic acid much hydrocyanic acid is evolved, but neither ammonia nor carbonic acid is perceptible. A peculiar acid containing chlorine, carbon, and azote, is formed, which is easily obtained by pouring a little muriatic acid upon fulminate of silver, until the filtered fluid is no longer rendered turbid by the acid. It possesses the following properties:

Its taste is sharp; it reddens litmus paper strongly; it does not precipitate nitrate of silver; neutralizes bases; and then possesses the property of colouring the perchloride of iron of a deep red colour; it acquires it also after some hours' exposure to the air, because a portion of it is decomposed which produces ammonia that saturates the other part; heat accelerates this decomposition. When combined with potash and evaporated to dryness, ammonia is obtained, and the residuum effervesces with acids, and precipitates nitrate of silver. 20·70 parts of fulminate of silver, accurately decomposed by muriatic acid, yielded 19·84 of chloride of silver, which were separated from the filter by means of ammonia.

The new acid being mixed with hydrocyanic acid, which, as is well known, precipitates nitrate of silver, we employed the following process to determine the quantity of chlorine which it contains. We added to the acid potash in excess, and towards the end of the evaporation, carried on in a platina crucible, we added nitre, and made it red-hot, to decompose the cyanogen; we afterwards saturated the pure alkali, and precipitated by nitrate of silver. The weight of the chloride of silver obtained amounted to 48·20 parts, which is almost two and a half times greater than the weight of the chloride obtained by decomposing the fulminate by muriatic acid. In another experiment, the two quantities of chloride of silver were in the proportion of 17·62 to 44·25, which are also those of 1 to 2·5.

If these results are correct, it cannot be admitted that all the chlorine exists free from hydrogen in the new acid; for the fulminate of silver, containing only 4 proportions of oxygen and 2 of cyanogen, could only take the hydrogen of 6 proportions of muriatic acid, supposing that all the cyanogen was changed into hydrocyanic acid. A portion of it must, therefore, remain in the new acid, and 7 proportions of chloride of silver are obtained: it is evident, therefore, that a part of the chlorine is combined with hydrogen in the new acid.

For the purpose of obtaining some information in this respect, we attempted to ascertain the quantity of hydrocyanic acid which is evolved when fulminate of silver is treated with muriatic acid.

A known weight of fulminate of silver was put into a three-necked bottle, fig. 6, with water placed in a salt-water bath, and afterwards muriatic acid was poured through the tube, *f*, upon the fulminate. In order to facilitate the volatilization of the hydrocyanic acid, a current of hydrogen gas was passed into the fluid, from a bottle, *a*, containing a mixture of zinc and sulphuric acid. The hydrogen gas passed through a tube, *d*, containing fragments of marble with a little water, and afterwards escaped through a solution of nitrate of silver contained in the receiver, *e*. We were in hope of obtaining cyanuret of silver; but to our great surprise no precipitation took place, although we were certain that the same solution of silver gave an abundant precipitate when hydrocyanic acid was poured into it.

Hydriodic acid acts upon fulminate of silver in the same way as muriatic acid. Hydrocyanic acid is disengaged, and a peculiar acid is formed which contains iodine, and possesses the property of precipitating perchloride of iron immediately of a deep-red colour.

When a current of sulphuretted hydrogen gas is passed through water containing suspended fulminate of silver, the fulminate is also decomposed; sulphuret of silver and a peculiar acid are obtained, of which sulphur is one of the elements, but no smell of hydrocyanic acid is perceptible.

This new acid has a sweetish taste; it immediately colours perchloride of iron of a deep-red colour; the solution may be evaporated, and concentrated without decomposition. Combined with potash and evaporated to dryness, it suffers no alteration.

22·68 of fulminate of silver, treated with sulphuretted hydrogen, produced sulphuret of silver, which, treated with nitric acid, and afterwards with muriatic, gave 21·73 of chloride of silver.

The new acid, saturated with potash, evaporated and heated to redness in a capsule of platina with nitre, saturated and precipitated by chloride of barium, produced 18·60 of sulphate of barytes, representing 22·89 of chloride of silver. These two quantities of chloride, not being very different, seem to admit of the conclusion, that in fulminic acid, the sulphur exactly replaces the oxygen atom for atom; but a second experiment indicated by sulphate of barytes a rather smaller proportion than the first.

Fluoric acid does not act upon the fulminate of silver; the cause of this is not to be found in the solubility of fluoride of silver, for fulminate of copper is perfectly decomposed by muriatic acid. This fact appears to us important in the hitherto somewhat problematical history of fluoric acid.

The three peculiar acids of which we have spoken as colouring the perchloride of iron of a deep-red colour, must possess a common principle as the cause of this property. It is worthy of remark, that the slightly fulminating *amer*, several properties of which have been detailed by M. Chevreul, and the sulpho-cyanic acid of M. Porret, give the same red colour to perchloride of iron.

Oxalic acid decomposes fulminate of copper and of silver; hydrocyanic acid and ammonia are produced; no effervescence is perceptible, which seems to prove that no carbonic acid is formed. Sulphuric acid gives similar results.

We may remark, with respect to the preparation of the alkaline fulminates, that fulminic acid having the property of forming very variable double salts, it is preferable in obtaining the double fulminate of silver and potash, for example, to decompose the fulminate of silver by chloride of potassium. It may be immediately obtained in a state of purity, by employing only exactly the quantity of chloride sufficient to precipitate half the silver combined with the fulminic acid, or rather a little less, since the fulminate of silver which is undecomposed, being but little soluble, would remain with the chloride of silver. Nevertheless the limit of the total decomposition of the fulminate of silver may easily be ascertained by using heat, because the fulminate being then slightly soluble, a precipitate is obtained, if any remain undecomposed, by adding a little of the chloride. We repeat that all the fulminates, single or double, detonate with great facility, even in water, and that glass stirrers should not be used to agitate any fluid, which contains them in a state of mixture. We accidentally detonated, by this method, fulminate of silver and barytes in a porcelain capsule; the accident fortunately produced no ill effects, because the greater part of the fulminate was suspended in the liquid, and it was scarcely warm; but without this union of circumstances, the consequences would have been terrible.

ARTICLE VI.

Analyses of the Chrysoberyls from Haddam and Brazil. By Mr. H. Seybert.*

In the summer of 1823, I visited Haddam, in the State of Connecticut. Among the various substances there collected was the Chrysoberyl, a mineral much esteemed on account of its rarity. It occurs disseminated in a coarse grained granite, in which the predominant ingredient is a white feldspar, which Prof. Berzelius regards as *Albite*, perfectly resembling that of Finbo. In the same granite this celebrated chemist observed the *Columbite*.† It is also associated with greyish quartz, manganesian garnet of a fine blood red colour, and a yellow granular substance, which some mineralogists supposed to be a variety of the cymophane; but from its inferior hardness and general chemical composition, I recognised it to be common beryl.

For the earliest chemical information concerning the chrysoberyl, we are indebted to Prof. Klaproth. He published his analysis of it in 1795,‡ and gave the following constituents of it, viz. alumina, 71·50; lime, 6; oxide of iron, 1·50; silica, 18; loss, 3. Berzelius presented us with a formula founded on this composition; § but from his experiments with the blowpipe he was led to conclude that it contained no lime, and that it was a subsilicate of alumina.‖ In this he was apparently confirmed by Prof. Thomson,** who quotes Klaproth's analysis, and states that he examined the mineral some years ago, but having accidentally lost his results, he was unable to publish them. He observes, however, that the only constituents he found were alumina, silica, and oxide of iron. When I was about to prepare the communication which I now have the honour to lay before the Society, a more recent analysis of the chrysoberyl of Brazil, by M. Augustus Arfwedson, was observed, by me, in Tilloch's Philosophical Magazine.†† He confirmed the results of Prof. Thomson, and considered the chemical composition of this substance to be—silica, 18·73; and alumina, 81·43, with a trace of oxide of iron.

The cymophane, from Haddam, was sent to M. Hauy by the late Dr. Bruce, in 1810, to have his opinion concerning its nature.* Previous to that period, the mineralogists in the United States supposed it to be *Corundum.* The late celebrated crystallographer observes, "La cymophane des Etats Unis a d'abord eté prisè pour une varieté de corindon. Effectivement elle se rapproche de ce mineral par sa dureté, par sa pesanteur spécifique, et même par le resultat de son analyse, qui a donné environ 72 parties d'alumine sur 100, avec 18 de silice, et 6 de chaux."† I was anxious to examine the cymophane found at Haddam, especially as M. Hauy does not name the author of the analysis he quotes. The specimen used for my experiments was of a pale green colour. It did not present any of the chatoyant appearance so remarkable in the variety from Brazil, and some specimens from Saratoga in New York, where it was lately discovered by Dr. Steel. Its specific gravity, by two trials, was 3·508 and 3·597. It is not magnetic, and before the blowpipe it is infusible. For a further description of the physical characters of this mineral, I refer to Hauy and Cleaveland.

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* Extracted from the second volume, New Series, of the Transactions of the American Philosophical Society, Philadelphia.

† Essai de l'Emploi du Chalumeau, p. 243.

‡ Beitrage, vol. i. p. 97.

§ Systeme de Mineralogie, p. 219,—C 4 S + 18 A 4 S.

‖ Essai de l'Emploi du Chalumeau, p. 325.

** Thomson's Chemistry, vol. iii. p. 213.

†† No. for November, 1823, p. 357.

Three grammes of the mineral were examined under the impression that Prof. Klaproth's analysis was accurately made. It was decomposed in the usual manner with four parts of caustic potash, and subsequently treated with diluted muriatic acid; but the solution was imperfect. The insoluble matter was collected on a filter, and it amounted to 25 or 30 per 100. It was repeatedly acted on in the same way, and each time it diminished in quantity, until the fourth experiment. It then weighed about fifteen-hundredths, and thereafter resisted all further efforts to render it soluble by these means. This residue was then boiled in concentrated sulphuric and muriatic acids, but neither of them dissolved more than one-third of it. These solutions were tested by different reagents, and greatly to my surprise, the addition of subcarbonate of ammonia occasioned a flocculent precipitate, which entirely redissolved in an excess of the alkaline subcarbonate. I immediately suspected the presence of glucina, but was much at a loss to explain its insolubility, until I observed Berzelius's analysis of the euclase;‡ in which he met with a compound of glucina and oxide of tin that obstinately resisted acids. He also met with refractory combinations of this earth and the oxides of manganese and cerium. I next endeavoured to dissolve the compound by the acid sulphate of potash; but this method did not succeed. I was not more successful with the nitric and nitromuriatic acids; nor could it be dissolved by means of boric acid. Berzelius having discovered columbium in the gangue of the cymophane from Haddam, the insoluble residue was tested for the oxide of that metal, but all my attempts were fruitless. At length, I supposed, that as barytes could be brought into contact with this substance more conveniently than potash at a high temperature; it might decompose it. With this view, a portion of the insoluble matter was exposed to a strong heat, during one hour, with six parts of nitrate of barytes in a platina crucible. The calcined mass was boiled in nitric acid. In this way nearly two-thirds of the matter that could not be entirely attacked in any other way, were dissolved. The same treatment was repeated, until nearly the whole of it was taken up, which happened after the fourth calcination. It was then no further acted on.

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* Annales du Museum d'Histoire Naturelle, tome xviii. p. 57.

† Traité de Mineralogie, second edition, vol. ii. p. 309.

‡ Nouveau Systeme de Mineralogie, p. 289.

After making numerous experiments on the matter that resisted nitrate of barytes and nitric acid, I ascertained that it was not acted on by alkalies nor acids when used separately, but after having been previously calcined with caustic potash, it readily dissolved in muriatic acid, yielding a solution of a pale-yellow colour, which gave a reddish precipitate with an infusion of galls, a deep-green precipitate with the hydrosulphate of potash, and a white precipitate with alkalies. Hence it was oxide of titanium.

After the barytes was separated with sulphuric acid, the nitric solutions were united, and treated with an excess of subcarbonate of ammonia. An abundant precipitate ensued, which entirely redissolved in the excess of subcarbonate. By ebullition it was again precipitated; and when calcined, it was in the form of a light white powder, possessing all the properties that characterise glucina. With the sulphuric and muriatic acids it formed very sweet astringent deliquescent salts. By caustic potash it was precipitated from its solutions, and the precipitate redissolved in the excess of the alkali. Klaproth and Arfwedson, in their analyses of the chrysoberyl from Brazil, considered the insoluble matter remaining, after they had treated the mineral with potash and muriatic acid, to be silica. This will explain why their results differ so essentially from mine.

After having thus satisfied myself of the composition of the residue above-mentioned, I resumed my preliminary experiments, and proceeded to examine the muriatic solution obtained from the treatment of the mineral with potash and muriatic acid. From this solution some silica was separated. A portion of the liquid was treated with caustic ammonia, and then tested for lime with oxalate of potash, but none of it could be detected. To the remaining liquor a considerable excess of subcarbonate of ammonia was added, and the precipitated matter was digested twenty-four hours. It was then separated by filtration, and the fluid was boiled till all the ammonia was expelled. No glucina was thus precipitated. Hence we conclude that the very small portion of titanium above-mentioned, rendered the whole of the glucina so refractory. The alumina precipitated by the subcarbonate of ammonia was mixed with a small quantity of oxide of iron. It was soluble in caustic potash, and with this alkali and sulphuric acid it gave regular octohedral crystals of alum. The liquor, when tested with phosphate of soda and ammonia, was found to contain no magnesia.

After the preliminary experiments, I commenced the following

Analysis of the Chrysoberyl from Haddam.

A. Five grammes of the mineral, reduced to small fragments in an iron mortar, were carefully porphyrised in one of agate, from which it acquired the additional weight of 0·13 grammes: The 5·13 grammes were then exposed to a red heat, and thereby suffered a diminution of 0·40 per 100.

B. The calcined mineral (A) was heated, during one hour, in the silver crucible, with caustic potash, and the product was treated with diluted muriatic acid; the solution was of a lemon-yellow colour. There remained a white insoluble residue, which after calcination weighed 1·47 grammes. It was repeatedly calcined with caustic potash, and treated with diluted muriatic acid, with the following results:

After the 2d experiment, it weighed 0·97 grammes.
3d 0·89
4th 0·85

By the fifth treatment it was not diminished, and then presented itself in the form of a light white powder, resembling pure silica in appearance.

C. The residue (B) was repeatedly strongly calcined with six parts of nitrate of barytes, and subsequently boiled with nitric acid.

After the 1st treatment, there remained 0·43 grammes.
2d 0·15
3d 0·06

And by the 4th operation only 0·01 gramme was dissolved.

The remaining 0·05 gramme was essayed in the manner related in the preliminary experiments, and thus proved to be oxide of titanium. Hence we have 1 per 100 of that oxide.

D. The nitric solutions were united and evaporated to dryness to expel the excess of the acid. The saline mass was dissolved in water, and after the barytes was separated with sulphuric acid, an excess of subcarbonate of ammonia was added to the solution. An abundant precipitate appeared, which entirely redissolved. The glucina was precipitated by ebullition. After edulcoration and calcination, it weighed 0·79 gramme, or 15·80 per 100.

E. The several muriatic solutions (B) were united and evaporated to a dry mass, which was treated with muriatic acid, and there remained 0·33 gramme of silica, from which deduct 0·13 gramme acquired from the agate mortar; and there will be 0·20 gramme, or 4 per 100 as a constituent of the mineral.

F. After the silica was separated from the liquid (E), the alumina and oxide of iron were precipitated by means of a great excess of subcarbonate of ammonia. After twenty-four hours, the liquor was separated from the yellowish precipitate, and was boiled, but no glucina was precipitated from it. The matter precipitated by the subcarbonate of ammonia consisted of 3·68 grammes of alumina, or 73·60 per 100, and 0·19 gramme of peroxide of iron, which, on account of the colour of the mineral, must be estimated as protoxide. The 0·19 gramme of peroxide is equivalent to 0·169 of protoxide, or 3·38 per 100.

The constituents of this chrysoberyl therefore are,

	Per 100 parts.
A. Moisture	0·40
C. Oxide of titanium	1·00
D. Glucina	15·80
E. Silica	4·00
F. Alumina	73·60
F. Protoxide of iron	3·38
	98·18
	100·00
Loss	1·82

As the preceding results differed so essentially from the analyses of the chrysoberyl from Brazil by Klaproth and Arfwedson, I determined to examine a specimen from that locality. 1·5 gramme was analyzed in the manner above-mentioned, and the following results were obtained:—

	Per 100 parts.
Water	0·666
Oxide of titanium	2·666
Glucina	16·000
Silica	5·999
Alumina	68·666
Protoxide of iron	4·733
	98·730
	100·000
Loss	1·270

In estimating these constituents according to the electro-chemical theory, I believe that the oxide of titanium, notwithstanding its important agency in the analytical experiments, must be regarded as an accidental ingredient, as well as the oxide of iron, which in some measure may have been derived from the iron mortar. As the cymophane of Brazil appears to be constituted more conformably to the hypothesis of chemical proportions than that of Haddam, the following calculation may be made, founded on its composition, which gives for the essential constituents of chrysoberyl,

			Per 100 parts.
Silica......	6·61	containing oxygen	3·32
Alumina....	75·75		35·38
Glucina	17·64		5·49

and very nearly corresponds with the following mineralogical formula, $A\ {}^4S + 2\ G\ A^4$.

Article VII.

*Of Poisons, Chemically, Physiologically, and Pathologically considered.**

Toxicology, or the history of poisons, forms one of the most important and elaborate branches of forensic medicine; and in tracing the subject through all its numerous and interesting relations to jurisprudence, we shall experience no small degree of gratification by observing, how greatly and progressively this obscure department of science has, within the last few years, been enlightened by the discoveries of chemistry and physiology.

The labours of the modern chemist, indeed, have enabled us to recognise and identify each particular substance by its properties and habitudes, with an infallible delicacy, which the physicians of a former age could scarcely have anticipated, and much less practised.

The physiologist, by an invaluable series of observations and experiments, has demonstrated the particular organ, or texture, upon which each individual poison exerts its energies; and the pathologist has been thus enabled to establish the mode in which it depraves the health, or extinguishes the life of an animal. Nor has the anatomist withheld his contributions upon this interesting occasion, for he has demonstrated the situation, extent, and intensity, of the organic lesions which result from the operation of these terrible agents upon the living body; and has pointed out several appearances which occur from natural causes, but which might be mistaken by the unskilful or superficial observer, for the ravages of poison. It remains for the forensic physician to converge into one focus the scattered rays which have thus emanated from so many points, and thereby to elucidate and determine the line of conduct which the medical attendant is called upon to pursue, for the relief of the patient suffering under the torments of poison, and for the establishment of the guilt or innocence of the party charged with the perpetration of a crime, which may be said to rob courage of its just security, while it transfers to cowardice the triumphs of valour. That engines so powerful and secret in their work of destruction should have universally excited the terror of mankind is a fact which cannot surprise us, and, when we consider how intimate are the relations between fear and credulity, we need not seek further for the solution of the many problems to which the exaggerated statements of ancient toxicologists* have given origin; the most extraordinary of those relate to the alleged subtlety of certain poisons, which was believed to be so extreme as to defeat the most skilful caution, and at the same time so manageable, as to be capable of the most accurate graduation; so that in short the accomplished assassin was not only thus enabled to ensure the death of his victim through the most secret and least suspicious agents, but to measure his allotted moments with the nicest precision, and to occasion his death at any period that might best answer the objects of the assassination. The writings of Plutarch, Tacitus, Theophrastus, Quintillian, and Livy, abound with such instances of occult and slow poisoning; most of which, however, notwithstanding the weight they may acquire from their testimony, bear internal evidence of their fallacious character. Plutarch informs us that a slow poison which occasioned heat, cough, spitting of blood, a lingering consumption of the body, and a weakness of intellect, was administered to Aratus of Sicyon. This same poison is also alluded to by Quintillian in his declamations. Tacitus† informs us that Sejanus caused a *secret* poison to be administered by an eunuch to Drusus, who in consequence gradually declined, as if by a consumptive disorder, and at length died. Theophrastus‡ speaks of a poison, prepared from aconite, that could be so modified as to occasion death within a certain period, such as two, three, or six months, a year, and even sometimes two years.

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* From Medical Jurisprudence, vol. ii. by Dr. Paris and J. S. M. Fonblanque, Esq.

To such an extent does the crime of poisoning appear to have been carried, about 200 years before the Christian æra, that, according to Livy,§ above 150 ladies, of the first families in Rome, were convicted and punished for preparing and distributing poison. The most notorious and expert character of this kind is handed down to us by the historians and poets under the name of Locusta, who was condemned to die on account of her infamous actions, but was saved in order that she might become a state engine, and be numbered, as Tacitus expresses it, "*Inter instrumenta regni.*" She was accordingly employed to poison Claudius by Agrippina, who was desirous of destroying the Emperor, and yet feared to dispatch him suddenly; whence a slow poison was prepared by Locusta, and served to him in a dish of mushrooms, of which he was particularly fond, "*Boletorum appetentissimus;*" but it failed in its effects, as we learn from Tacitus, until it was assisted by one of a more powerful nature. "*Post quem nihil amplius edit.*" This same Locusta prepared also the poison with which Nero dispatched Britannicus, the son of Agrippina, whom his father Claudius wished to succeed him on the throne. This poison appears to have proved too slow in its operation, and to have occasioned only a dysentery. The Emperor accordingly compelled her by blows and threats to prepare in his presence one of a more powerful nature, and as the tale is related by Suetonius, it appears that it was then tried on a kid, but as the animal did not die until the lapse of five hours, she boiled it for a longer period, when it became so strong as instantaneously to kill a pig to which it was given. In this state of concentration it is said to have dispatched Britannicus as soon as he tasted it.* Vide Tac. An. 13. s. 15, 16. Now it would clearly appear from these statements that Locusta, avowedly the most accomplished poisoner of ancient Rome, was wholly incapable of graduating the strength of her poisons to the different purposes for which they were applied.

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* The study of poisoning appears to have been of considerable antiquity. Ulysses sought poison for his weapons from Ilus, "φαρμακον ανδροφονον". Od. l. i. v. 261; but the conscientious pharmacopolist refused to furnish his dangerous preparations to the wily chief.

† Taciti Annal. lib. iv. c. 8.

‡ Hist. Plant. lib. ix. c. 16, p. 189.

§ Lib. viii. c. 18.

The records of modern times will furnish examples no less atrocious than those we have just related. Tophana, a woman who resided first at Palermo, and afterwards at Naples, may be considered as the Locusta of modern history; she invented and sold those drops so well known by the names of Aqua Toffania; Aqua della Toffana; Acquetta di Napoli, or simply Acquetta. This stygian liquor she distributed by way of charity to such wives as wished for other husbands; from four to six drops were sufficient to destroy a man, and it was asserted that the dose could be so proportioned as to operate within any given period.† It appears that in order to secure her poison from examination, she vended it in small glass phials, inscribed "*Manna of Saint Nicolas Bari,*" and ornamented the vessel with the image of the Saint. Having been put to the rack she confessed that she had destroyed upwards of 600 persons, for which she suffered death by strangulation in the year 1709.‡ In 1670 the art of secret poisoning excited very considerable alarm in France; the Marchioness de Brinvillier, a young woman of rank and great personal attractions, having intrigued with, and subsequently married an adventurer named Saint Croix, acquired from him the secret of this diabolical act, and practised it to an extent that had never before been equalled. She poisoned her two brothers through the medium of a dish at table. She also prepared poisoned biscuits, and to try their strength she distributed them herself to the poor at the Hotel Dieu. Her own maid was likewise the subject of her experiments. To her father she gave poisoned broth, which brought on symptoms characteristic of those induced by corrosive sublimate. Her brothers lingered during several months under much suffering. The detection of this wretch is said to have been brought about in the following manner. Saint Croix, whenever engaged in the preparation of his poisons, was accustomed to protect himself from their dangerous fumes by wearing a glass mask, which happening to fall off by accident, he was found dead in his laboratory.* A casket directed to the Marchioness, with a desire that in case of her death it might be destroyed unopened, was found in his chamber, a circumstance which in itself was sufficient to excite the curiosity and suspicion of those into whose hands it fell. The casket was accordingly examined, and the disclosure of its contents at once developed the whole plot, and finally led to the conviction of this French Medea, who, after a number of adventures and escapes, was at length arrested and sent to Paris, where she was beheaded, and then burnt, on the 11th of July, 1676. The practice of poisoning, however, did not cease with her execution, and it became necessary in 1679 to establish a particular Court, for the detection and trial of such offenders; which continued for some time to exert its jurisdiction under the title of Chambre de Poison, or Chambre Ardente.

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* For the ingenious mode in which this poison was administered, see Tacitus. The prince having called for a cup of wine, it was purposely presented too hot; he desired cold water to be added to it, and the opportunity was then taken to infuse the poison. By this stratagem the taster ("calida gelidæque minister." Juv. Sat. v. v. 63) escaped its effects, in which he must otherwise have participated with Britannicus.

† The reader will find a very interesting account of this diabolical woman in Labat's Travels through Italy, and also in Beckman's History of Inventions.

‡ Hoffman Medicin. Rational.

With respect to the secret modes in which poisons have been supposed capable of acting, mankind have ever betrayed the most extravagant credulity, of which the numerous tales upon record afford ample proof; such as that reported of Parasapis by Plutarch, from Ctesias, in his life of Artaxerxes, who, it is said, by anointing a knife on one side by poison, and therewith dividing a bird, poisoned Statira with one half, and with the other regaled herself in perfect security. We are also told of Livia who poisoned the figs on a tree which her husband was in the habit of gathering with his own hands. Tissot informs us that Johu, king of Castille, was poisoned by a pair of boots prepared by a Turk; Henry VI. by gloves;† Pope Clement VII.

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* This story, if we mistake not, suggested to the successful author of Kenilworth the tragic death of his alchymist.

† The belief in the possibility of poisoning by the vestments is very ancient, as is shown by the fabled death of Hercules.

——————— "Capit inscius heros:
Induiturque humeris Lernææ virus Echidnæ.
———————————————————
———————————————————
Incaluit vis illa mali; resolutaque flammis;
Herculeos abiit late diffusa per artus."

Ovid. Metam. lib. ix. v. 157.

by the fumes of a taper;[*] and our king John in a wassail bowl, contaminated by matter extracted from a living toad. To these few instances of credulity may be added the offer of the priest to destroy queen Elizabeth by poisoning her saddle,[†] and the Earl of Essex, by anointing his chair.

Incredible and absurd as these opinions now appear, they continued until a late period to alarm mankind, and to perplex and baffle judicial investigations; even Lord Bacon in his charge against the Earl of Somerset for the murder of Sir Thomas Overbury, in the Tower, seemed to give credit to the story of Livia, and he seriously stated, that "Weston chased the poor prisoner with poison after poison; poisoning salts, poisoning meats, poisoning sweetmeats, poisoning medicines and vomits, until at last his body was almost come, by the use of poisons, to the state that Mithridates's body was by the use of treacle and preservatives, that the force of poisons was blunted upon him;" Weston confessing, when he was reproached for not dispatching him, that he had given enough to poison twenty men.[‡] The power of so graduating the force of a poison as to enable it to operate at any given period seems to have been considered possible by the earlier members of the Royal Society; for we learn from Spratt's history of that learned body, that very shortly after its institution, a series of questions were drawn up by the direction of the Fellows, for the purpose of being submitted to the Chinese and Indians, viz. "Whether the Indians can so prepare that stupifying herb, Datura, that they make it lie several days, months, years, according as they will have it, in a man's body, without doing him any hurt, and at the end kill him without missing half an hour's time?"

That mankind were, in a very early stage of their existence, not only acquainted with the deadly effects of certain natural substances when applied in minute quantities, but that they availed themselves of such knowledge for the accomplishment of the worst purposes, is very satisfactorily shown by the records of sacred as well as profane authors. But such is the ambiguity of ancient writers upon this subject, and so intimately blended are all their receipts with the practices of superstition, that every research, however learned, into the exact nature of the poisons which they employed, is necessarily vague and unsatisfactory. Of this one fact, however, we may be perfectly satisfied, that they were solely derived from the animal and vegetable kingdoms, for the discovery of mineral poisons was an event of later date; owing however to the defect of botanical nomenclature, it is even doubtful whether the plants which are designated by the terms cicuta, aconitum, &c. in ancient authors, were identical with those we designate by the same names. (See Pharmacologia, fifth edit. vol. i. p. 66.) With respect to the poisons of Locusta, all cotemporary writers speak of the venom of the toad as the fatal ingredient of her potions, and in the Alexipharmaca of Dioscorides we find the symptoms described, which are said to be produced by it;* but what is very extraordinary, the belief of the ancients on this matter was all but universal. Pliny is express on the subject; Ætius describes two kinds of this reptile,† the latter of which, as Dr. Badham has suggested, was probably the frog, as well from the epithet, as that he ascribes deleterious powers only to the former. It is scarcely necessary to observe that this ancient belief has descended into later times; we find Sir Thomas Browne treating such an opinion as one of the vulgar errors; and we have before alluded to the legend of king John having been poisoned by a wassail bowl in which matter extracted from a living toad was said to have been infused. In still later times, we have heard of a barrel of beer poisoned by the same reptile having found its way into it. Borelli and Valisnieri maintain that it is perfectly harmless, and state that they had seen it eaten with impunity. Spielman‡ expresses the same opinion, "*Minus recte itaque effectus venenati a bufonibus metuuntur.*" Franck,§ on the contrary, accuses Gmelin of too much precipitancy in rejecting the belief respecting toad-poison.‖ Modern naturalists recognise no poisonous species of toad; even the most formidable of the species, to appearance, that of Surinam, is said to be perfectly harmless.

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[*] Quæst. Med. Leg.

[†] Sir Edward Coke in the trial of Sir John Hollis.

[‡] Bacon's Works, vol. ii. p. 614.

If we may venture to offer a conjecture upon this subject, we are inclined to consider the origin of this opinion to have been derived from the frequency with which the toad entered into the composition of spells or charms, into philtres or love potions; and which, like the bat and the owl, most probably derived its magical character from the gloom and solitude of its habitation. Shakspeare has accordingly introduced this reptile into the witches' enchanted cauldron, in Macbeth.

> "Round about the cauldron go;
> In the poison'd entrails throw.
> Toad that under coldest stone
> Days and nights hast thirty-one
> Swelter'd venom sleeping got,
> Boil thou first i' the charmed pot!"

This opinion receives further strength when it is considered how frequently poisons were administered under the insidious form of charms or incantations.**

* "επιφερειν οιδηματα σωματος, μετα ωχροτητος επιτεταμενης. δυσπνοειν και δυσωδια οζεσθαι το στομα, και λυγμος αυτοις ἐπεται, ενιοτε δε και σπερματος απροαιρετος εκκρισις."

† 1. κωφος η αφθογγος; 2. φωνητικος.

‡ Instit. Mater. Medic. p. 176.

§ Manuale di Tossicologia, p. 79, 245.

‖ See also Instituzioni di Med. For. di G. Tortosa, vol. ii. p. 67, and authorities there cited.

** This fact may be illustrated by ancient as well as modern records; from the poisoned tunic of the Centaur Nessus, to the treacherous powders of the diabolical Mary Bateman.

It has, however, been shown by late experiments that the toad has, under particular circumstances, the power of ejecting from the surface of the body an acrid secretion which excoriates the hands of those that come in contact with it; and this fact may, perhaps, have assisted in supporting the general belief respecting the poisonous nature of this reptile. Pelletier has ascertained, that this corrosive matter, contained in the vesicles which cover the skin of the common toad (*Rana Bufo*), has a yellow colour, and an oily consistence, and to consist of,—1st, an acid partly united to a base, and constituting 1-20th part of the whole; 2d, very bitter fatty matter. 3d, an animal matter bearing some analogy to gelatine.

It would also appear from the writings of Dioscorides, Galen, Nicander, Ætius, Ælian, and Pliny, that the ancients derived a very energetic poison from the sea hare, *Lepus Marinus*,—the *Aplysia Depilans* of Linnæus; and, if we may credit Philostratus, it was with such a poison that Titus was killed by Domitian.

There is, however, ample ground for supposing that the poisons of the ancients were, for the most part, obtained from the vegetable kingdom, and from the class of narcotic plants;[*] that they were compounded of a great variety of such ingredients, together with others that were quite inert and useless, and which merely served to disguise their composition.

Ancient writers also allude to the blood of the bullock as a poison; Themistocles is said by Plutarch to have destroyed himself by this fluid; and Strabo states, that Midas died of drinking the hot blood of this animal, which he did, as Plutarch mentions, to free himself from the numerous ill dreams which continually tormented him. Some historians assign the death of Hannibal to the same draught.

With respect to the poisons employed by Tophana, the Locusta of modern days, and her infamous successors, there is less doubt; arsenic, corrosive sublimate, sugar of lead, and antimony,[†] were among the most powerful of their instruments of torture and death. According to the declaration of the Emperor Charles VII. to his physician Garelli, the *Aqua Toffania* was a solution of arsenic in *Aqua Cymbalariæ*.[‡] Dr. Hahneman considered its basis to have been an arsenical salt. Others have, with little probability, regarded opium and cantharides as the active ingredients. Franck,[*] speaking of the *Aqua Toffania*, agrees with Gmelin,[†] that it is no other than a solution of arsenic. The *Pulvis Successionis*, another instrument of death, whose title announces the diabolical intention with which it was administered, has been supposed to have been a preparation of lead; while others have considered it to have consisted of diamond dust, and to have acted mechanically.

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[*] Theophrast. Hist. Plant. lx. c. 16. Strabo mentions the action of the Laurocerasus, as a poison, and observes that it occasions a death like that of epilepsy.

[†] All these substances were found in the casket of Saint Croix.

[‡] Gerarde, in his Herbal, considers the Cymbalaria to be the Pennywort of which he describes two varieties, viz. the Wall-pennywort, and the Water-pennywort; and he blames the "ignorant apothecaries," for using the latter instead of the former, as extremely dangerous and destructive to life. Modern botanists consider it as an Antirrhinum,—A. Cymbalaria. Lin. i. e. Ivy-leaved Toad-flax. We are not aware of any part of this genus being poisonous. The A. Linaria, common Toad-flax, appears to be the only one to which any medicinal virtues have been ascribed. Linnæus, however, says (Flor. Suec.) that this plant is used as a poison to flies.

Having thus noticed a few of the more remarkable and interesting features in the literary history of Toxicology, we shall proceed to consider the subject of poisons, in relation to their operation.

A poison (*Toxicum, Venenum, Virus*), has been very correctly defined by Gmelin to be a substance which, when administered internally, or applied externally in a small dose, impairs the health, or destroys life. This definition is adopted by Mead, Sproegel, Plenck, and Tortosa, and is to be preferred to every other,[‡] not only for its simplicity, but for its independence of any theory relative to the *modus operandi* of such agents. But it will be seen that, by accepting this definition, we are necessarily led to admit the fact, that poisoning may be acute, or chronic; that is to say, that it may at once destroy life, or produce a disease which can be protracted to any indefinite period. After the erroneous and vague notions which have been entertained upon the subject of "Slow poisons," it is highly essential that the latitude of our belief should be accurately ascertained, and the precise meaning of our terms defined.

Of Slow, Consecutive,[§] *and Accumulative Poisoning.*

1. *Slow Poisons.*—According to the popular acceptation of the term, they may be defined, substances which can be administered imperceptibly; and a single dose of which will operate so gradually, as to shorten life like a lingering disease; their force, at the same time, admitting of so nice an adjustment as to enable the artist to occasion death at any required period. We have now to inquire how far such alleged powers are consistent with the known laws of physiology. It cannot be denied that certain substances have been introduced into the alimentary canal, where they have remained for an indefinite period, with-

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[*] Man. de Toxicol.

[†] Hist. General de Venen. mineral.

[‡] Boerhaave gives us the following definition. "*Venenum dico omne illud quod ingestum vel applicatum corpori, talem in corpore humano mutationem excitat, quæ per ipsam eam mutationem non superatur. Medicamentum prætorea in eo differt, quod ipsa, quam facit mutatio, in sanitatem tendat, venenum vero corpus mutat, ut ex sano ægrum fiat, aut cadaver.*" (Prælect. Acad. T. vi. p. 283.) Hoffmann has furnished us with a definition less exceptionable than the foregoing, but still inferior to that of Gmelin. "*Alit natura res, quæ exigua mole et summa partium tenuitate, brevi tempore concentum atque ordinem motuum vitalium perpertunt, vel plane destruunt; et hæ vocari solent Venena.*" (M. R. S. T. II. p. 88.)

[§] We have adopted this term, as one that has been in previous use, although we are by no means satisfied that a more expressive word might not be found.

out occasioning the slightest inconvenience, and at length excited a disease that has terminated fatally; in the London Medical and Physical Journal for February, 1816, a case is related in which death was occasioned by a chocolate-nut having lodged in the entrance of the appendix vermiformis; and in the Edinburgh Medical and Surgical Journal for July, 1816, we have an analogous case, communicated by Dr. Briggs of Liverpool, where the Appendix cæci sphacelated, owing to the irritation of a human tooth which was found sticking in its cavity. Mr. Children has lately communicated to the Royal Society a case where a concretion in the colon produced death; upon examination it was found to contain a plum-stone, as a nucleus, and to consist of a fine fibrous vegetable substance, from the inner coat enveloping the farina of the oat, and which was derived from the oatmeal upon which the deceased had fed. (Phil. Trans. 1822.) However disposed we may feel, by a forced construction of the term, to consider such agents as *slow* poisons, it is very evident that they can rarely have been made subservient to the purposes of secret poisoning; although a case occurred in the practice of the author,* in which a girl swallowed six copper pence for the avowed purpose of destroying herself; the coin produced a disease which remained chronic for a very considerable period, when, after a lapse of five years, they were voided, and the young woman recovered. A similar attempt was also made by Theodore Gardelle, after his conviction for the murder of Mrs. King (*vide antè*); he swallowed a number of halfpence, for the purpose of destroying himself, but without any ill effect. Dr. Baillie, in his "Morbid Anatomy," relates an instance where five halfpence had been lodged in a pouch in the stomach for a considerable time, without occasioning any irritation; and Mr. A. Thomson has also furnished us with two analogous cases in children, in one of which the copper coin remained six months in the intestines, and in the other two months. These facts furnish sufficient data to enable the practitioner to appreciate the degree of danger attendant upon such agents, and to determine how far they can ever become successful instruments in the hands of the assassin.†

But it has been supposed that certain bodies, as glass, enamel, diamonds,‡ agates, smalt, &c. when administered in the form of powder, so lacerate the membranes of the stomach by the sharpness of their particles, as slowly to destroy life; and upon the same principle, it has been asserted, that human hair, chopped fine,* constitutes the active ingredient of a slow poison frequently employed in Turkey, and that it induces by irritation a chronic disease resembling cancer. With respect to the danger arising from the ingestion of diamond dust, enamel powder, powdered glass, and the like, there still may be said to exist some difference of opinion. Caldani, Mandruzzato,† and M. Le Sauvage, have reported experiments made upon men and inferior animals, in which no bad consequences followed the administration of such bodies; whereas Schurigius‡ and Cardanus§ cite instances where persons have died of ulcerations of the stomach from such causes; and this opinion receives the support of Plouquet,‖ Stoll;** Gmelin,†† Fodere,‡‡ Mahon,§§ Franck,‖‖ and many others. The modern pathologist will not find much difficulty in reconciling such conflicting testimony. The experimentalist may administer mechanical substances a thousand times without producing any ill effects, while, under certain circumstances, the most trivial body may lodge in the intestines and produce death; but surely the occasional occurrence of such accidents ought not to confer the general title of *poison* upon the substances which may happen to produce them.

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* This case is detailed in his 'Pharmacologia,' under the article Cupri Sulphas.

† See an interesting paper by Dr. Marcet, in the 12th volume of the Medico-Chirurgical Transactions, entitled, "Account of a Man who lived Ten Years after having swallowed a number of Clasp Knives."

‡ In the reign of Louis XIV. Henrietta, Duchess of Orleans, is said to have been poisoned by diamond-dust mixed with powdered sugar. The same substance is enumerated among other extraordinary poisons, as having been administered in the case of Sir Thomas Overbury.

Having thus disposed of a considerable number of bodies, which have been classed as *slow* poisons, we may proceed to observe that most of the other substances which have found a place in the same division, appear to us to deserve consideration under a very different head, and that we shall get rid of much obscurity by adopting the following arrangement.

2. *Consecutive Poisoning.*—Where the patient, having recovered from the acute effects occasioned by the ingestion of a single dose of poison, subsequently suffers a series of symptoms from the injured structure to which it had given origin. By referring to our definition of *slow* poisoning, we shall at once perceive the striking and important distinction between that and *consecutive* poisoning. The following case, related by M. Orfila, may serve as an illustration. Maria Ladari drank by mistake a spoonful of aquafortis, the most violent symptoms supervened, but which by judicious treatment gradually subsided, when at length she passed by stool a long membranous substance, rolled up, and which represented the form of the œsophagus and stomach, and which, in fact, was found to be the interior membrane of these organs; from that moment the sen-

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* Old women in the country recommend the same remedy for the destruction of worms; probably the medicine and the poison may be equally effective.

† Saggi Scientif. e Letter. dell' Accademia di Padova, tom. iii. p. 11, p. 1.

‡ Chylologia.

§ De Venenis.

‖ Comment. super Homicid. p. 177.

** Ratio Medendi. Part VI. p. 60.

†† Hist. General de Venenis Mineral.

‡‡ Med. Leg. tom. ii. p. 170.

§§ Tom. ii. p. 346.

‖‖ Man. de Toxicol.

sibility of the digestive organs became excessive, and two months after the accident she experienced a sudden shock and died. Mr. Tartra, in observing upon cases of this kind, asserts that the symptoms produced at first by the nitric acid decrease insensibly; and that at the end of a certain period, the internal membrane of the digestive canal is struck with death, and thrown off, and the person dies of a marasmus. Fordyce[*] relates the case of a woman who was subject to cholics for the space of thirty years, in consequence of having *once* taken an infusion of the pulp of colocynth prepared with beer. This was undoubtedly an extraordinary instance of idiosyncrasy, but it is probable that some organic lesion was occasioned by its operation, to which the subsequent suffering is to be referred. We have hitherto only considered the effects that may arise from the ingestion of a *single* dose of poison, but there are numerous and very interesting cases in which fatal results have been produced by the repetition of small doses at various intervals. We therefore propose a third, and new subdivision of our subject, viz.

3. *Accumulative Poisoning.*—By the repeated administration of a substance in doses, of which no single one could occasion harm; but which, by gradually accumulating in the system, ultimately occasions disease and death.

The familiar operation of mercury will at once suggest itself to the physician, as a striking illustration of that species of poisoning which we have ventured to name *Accumulative*, and to the forensic student the effects of this metal, in reference to such a quality, will form a more than ordinary object of interest, as involving questions which have frequently embarrassed judicial inquiry; as, for instance, Whether it can lie dormant any considerable time without betraying its effects upon the constitution, and, having displayed its powers, and the symptoms having subsided, viz. salivation, &c., Whether they can be renewed without a fresh application of the substance? See Corrosive Sublimate.

To how many substances this power of accumulation extends is at present not well understood. It may occur in those that act by absorption, and in those whose action is wholly local. Arsenic, digitalis, and several of the narcotic plants, as hemlock, may undoubtedly occasion serious mischief in this manner, as the author has more fully explained in another work,[†] and we have lately heard of several fatal cases arising from accumulated masses of magnesia in the *primæ viæ*, from the habitual use of small doses of that earth.

The history of many of the arts, especially those of metallurgy, would furnish also abundant examples of this kind of poisoning.

These few facts are, we trust, sufficient to authorise the foregoing arrangement, and we apprehend that the adoption of the distinctions, upon which it is founded, will be of great service in establishing fixed and definite notions with regard to the *chronic* operation of poisons. It may perhaps be useful to present the reader with a synoptical recapitulation of the subject.

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[*] Fragmenta Chirurg. et Med. p. 66.

[†] Pharmacologia, fifth edit. vol. i. p. 384.

A Slow Poison.—A single dose is sufficient; which produces upon its administration no sensible effect, but gradually undermines the health.

A Consecutive Poison.—A single dose is sufficient; producing the most violent symptoms, very shortly after its ingestion, but which gradually subside, and the patient is supposed cured; when, at some future period, death takes place from the organic lesions that had been occasioned.

An Accumulative Poison.—Many doses are required; the effects being produced by the repetition of doses which would, *individually*, be harmless.

There still remains another point of view in which it is essential to regard the operation of a poison, in order to establish a distinction between those substances which, in a given dose, will destroy life under every circumstance of constitution, and those which occasion death in consequence of some constitutional peculiarity in the individual to whom they may have been administered, and which are innocuous to the general mass of mankind; the gradations by which food, medicine, and poison, are thus enabled to branch into each other cannot be defined, because the circumstances with which they are related, defy generalization. The distinction, however, must be acknowledged and preserved, and we know no terms better adapted for expressing it than those of *Absolute* and *Relative* poisons; and our readers are accordingly requested to receive them in conformity with this explanation, whenever they occur in the following pages. Every work professing to treat the subject of poisons, abounds with instances, in which articles that, by universal consent, are considered innocuous, have occasioned the most direful effects. Morgagni relates a case of a person who died from eating bread made with the farina of the chesnut. Dr. Winterbottom[*] says that he is subject to severe nettle-rash after eating sweet almonds. Schenkius relates a case in which the general law of astringents and cathartics was always reversed. Donatus tells us of a boy whose jaws swelled, whose face broke out in spots, and whose lips frothed, whenever he eat an egg: we might add many more examples, but it is needless to encumber a subject with illustrations which is already so obvious and indisputable. Nor do the anomalies of constitutional idiosyncrasies end here, for they not only convert food into poison, but they change poison into food, or at least into a harmless repast. The most extraordinary exemplification of this on record is contained in the history of the old man at Constantinople, as related by M. Pouqueville, physician to the French army in Egypt, and who was a prisoner at Constantinople in the year 1798. "This man," says he, "was well known all over Constantinople, by the name of Suleyman Yeyen, or Suleyman, the taker of corrosive sublimate. At the epoch when I was there he was supposed to be nearly 100 years old, having lived under the Sultans Achmet III. Abdul Hamet, and Selim III. He had in early life habituated himself to taking opium; but, notwithstanding that he constantly increased the dose, he ceased to feel from it the desired effect, and then tried sublimate, the effects of which he had heard highly spoken of; for thirty years this old man never ceased to take it daily, and the quantity he could now bear exceeded a drachm. It is said, at this epoch he came into the shop of a Jewish apothecary, and asked for a drachm of sublimate, which he swallowed immediately, having first mixed it in a glass of water. The apothecary, terrified, and fearing that he should be accused of poisoning a Turk, immediately shut up his shop, reproaching himself bitterly with what he had done; but his surprise was very great, when the next day the Turk came again, and asked for a like dose of sublimate."

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[*] See Medical Facts and Observations, vol. v.

[†] See M. Pouqueville's "Voyage de Morée;" also Mr. Thornton's Travels; and Notes to Lord Byron's Childe Harold's Pilgrimage.

Morbid states of the body may also exist which are capable of resisting to a certain extent, or of modifying, the violent operation of particular poisons. In the history of the Royal Academy of Sciences for 1703, a case is related of a woman, who being tired out by a protracted dropsy, under which her husband had suffered, *charitably* administered to him fifteen or twenty grains of opium with the intention of dispatching him; but the dose immediately produced such copious evacuations by sweat and urine, that it restored him to health. This relation will immediately recall to the recollection of the classical reader the story recorded by Plutarch, in his life of Crassus, of Hyrodes king of the Parthians, who having fallen into a dropsical complaint had poison (aconite) administered to him by his second son, Phraates, but which, instead of destroying the king, as intended, cured his disease. The son, however, having thus failed in his attempt, shortly afterwards smothered his father with his pillow.

ARTICLE VIII.

Speculations and Inquiries respecting the Action and Nature of certain Compounds of Sulphur.

The cases in which sulphur decomposes water, and combines with one or both of its elements, may, perhaps, be reduced to three:

1. The action of a metallic sulphuret.
2. The action of a metallic oxide and sulphur.
3. The action of a metallic oxide and a metallic sulphuret.

The simplest case is that of putting sulphuret of potassium into water. In this case water is decomposed, the oxygen forming potash with potassium, and the hydrogen hydrosulphuric acid with the sulphur; and supposing that an atom of sulphuret of potassium is decomposed, there must of course be formed an atom of hydrosulphate of potash consisting of

1 atom hydrosulphuric acid (1 + 16) .. = 17
1 atom potash (8 + 40) .... = 48

65

When potash and sulphur are boiled together, the action is of course more complicated. Water must be decomposed, for an acid added to the solution evolves hydrosulphuric acid, and sulphur is precipitated. Now hydrosulphate of potash is not decomposed by an acid so as to precipitate sulphur, and moreover, although water is decomposed, what becomes of its oxygen? Does the following take place?

1 atom hydrogen .... = 1
1 atom sulphur.................... = 16

Hydrosulphuric acid 17

And does the atom of oxygen unite with another atom of sulphur to form hyposulphurous acid? Thus

1 atom oxygen........................ 8
1 atom sulphur........................ 16

Hyposulphurous acid.................. 24

This appears to be probable, for it will readily be granted that oxygen is not evolved from the decomposition of the water, while its hydrogen is forming hydrosulphuric acid. The oxygen cannot unite with potash, for that is already an oxide, and it cannot convert it into peroxide, for that is decomposed by water. As then in this case both the elements of the water appear necessarily to combine with the sulphur, there are, perhaps, formed hydrosulphate and hyposulphite of potash. But here again a difficulty occurs: when hydrosulphate and hyposulphite of potash are treated with an acid, although hydrosulphuric acid must be given out by the decomposition of the former salt, neither of them, I believe, yields sulphur, which is well known to be precipitated from the solution of sulphur in potash. Is there a third compound formed, which is merely a solution of sulphur in potash, and in which no water is decomposed? Is there in fact formed a real sulphuret of potash? This appears to me probable; in which case we should have, by dissolving sulphur in a solution of potash,

Hydrosulphate of potash,
Hyposulphite of potash,
Sulphuret of potash.

Without the existence of the last compound, I do not see how sulphur could be precipitated by an acid; and unless the first existed, hydrosulphuric acid could not be evolved by the same means; and it seems requisite to suppose the existence of the second in order to dispose of the oxygen.

This subject will, perhaps, admit of illustration by observing what happens when chlorine is passed into an aqueous solution of potash. The water is decomposed, its hydrogen combines with chlorine to form hydrochloric, and its oxygen with another portion of chlorine to form chloric, acid; this then is perfectly analogous to what I have supposed to occur with sulphur. It might indeed be imagined that the sulphur is converted into sulphuric acid instead of hyposulphurous; but this does not happen, for the solution when decomposed by muriatic acid affords no traces of the sulphuric on the addition of a barytic salt.

Whether a compound of chlorine and potash exists, I know not; but chloruret of lime is a well-known compound, and if chlorate, and consequently hydrochlorate of lime can also be formed by the action of chlorine upon water and lime, then the parallel between the actions of chlorine and sulphur will be complete; we should have hydrosulphate, hyposulphite, and chloruret of lime.

When sulphuret of antimony is boiled in solution of potash, what happens? We have sulphur, a metallic oxide, and a metal, operating upon and decomposing water; and when soda is used instead of potash (which is the same for the argument), a crystallized salt is obtained, which consists of hydrosulphuric acid, soda, and oxide of antimony. When sulphuric acid is added to this solution, hydrosulphuric acid is given out, and sulphate of soda formed, by the decomposition of the hydrosulphate of soda; but the hydrosulphate of antimony not being decomposed by the sulphuric acid and (unless when it forms a double salt), being insoluble, it is precipitated of an orange colour, consisting, there is no doubt, of hydrosulphuric acid and oxide of antimony. Now it is by no means easy to discover what happens in this case. Let us suppose we are operating upon an atom each of potash, antimony, and sulphur; if only 1 atom of water be decomposed, then protoxide of antimony, as we have reason to suppose is the case, would be formed by the union of 44 of antimony with 8 of oxygen. We have then 16 of sulphur to combine with 1 of hydrogen to form 17 = 1 atom hydrosulphuric acid; but this would be sufficient only to saturate either 48 of potash = 1 atom, or 52 of protoxide of antimony = 1 atom. This is a difficulty; but as a double salt is evidently formed, may we not suppose that the metallic oxides, oxide of potassium, and oxide of antimony, form sub-bisalts with the sulphuretted hydrogen? or so to speak, that they each take half an atom? We should then have a salt consisting of

1 atom hydrosulphuric acid .... = 17
1 atom potash .... = 48
1 atom oxide of antimony.......... = 52

It can hardly be admitted that more than one atom of water is decomposed; for in that case we should increase the difficulty; since not only would there be 2 atoms of hydrogen to combine with 1 of sulphur; but either hyposulphurous acid, or peroxide of antimony, must be formed at the same time: now for the former the sulphur is evidently insufficient, and peroxides lose oxygen by exposure to the action of hydrosulphuric acid.

The action of hydrosulphuric acid upon metallic oxides is reducible also to three cases. First, when they combine without either being decomposed, as when potash unites with it to form an hydrosulphate. In this case neither the hydrosulphuric acid yields its hydrogen, nor does the oxide give up its oxygen. The same appears to be the case with oxide of antimony, excepting that the compound is insoluble in water. The second case is when the hydrosulphuric acid is decomposed, but without precipitating the metal from solution. This happens when sulphuretted hydrogen is passed into a solution of peroxide of iron. The excess of oxygen combines with the hydrogen of the hydrosulphuric acid, the peroxide becomes protoxide, and sulphur is precipitated. The third case is that of adding hydrosulphuric acid to solution of lead, copper, &c. In these cases water appears to be formed by the union of the hydrogen of the hydrosulphuric acid, with the oxygen of the metallic oxide, and the consequence is that a metallic sulphuret is precipitated.

These hypothetical ideas are thrown out in the hope that they may lead to experiments for the purpose of elucidating an important but yet obscure branch of chemical science; and it may be observed, if these speculations are just, that the discovery of hyposulphurous acid is one of considerable importance in elucidating this subject.

Article IX.

On the Coliseum at Rome. By T. R. Underwood, Esq. MGS.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Paris, May* 24, 1821.

As I am not aware that the following circumstance has been hitherto noticed, I shall beg the favour of the insertion of it in your journal.

It is generally supposed that the ground plan of the external wall of the Coliseum at Rome is an ellipsis. This, however, is not the case, as I discovered, when examining it in September, 1802, that the plan of the external surface of the walls between the columns was a *straight line,* and not a curve; so that this vast amphitheatre is in fact a polygon of 80 sides. The difference in the effect of the whole, between this and an ellipsis, is not sensible in a general view, the angles of the polygon being covered by the columns; but the saving of time and expence in constructing the arches circular in a curved plan must, as every architect well knows, have been very considerable indeed. The entablatures, however, over the columns are ellipses, which is the source of the error respecting this building.

On my return from Italy such was the force of prejudice, even in professional men, English as well as French, in favour of the supposed ellipsis, that I could not obtain any credit for this statement, nor even induce any one who visited Rome, by accurate examination, to confirm or confute it, till M. A. Brongniart, Member of the French Institute, on his visit to Italy in 1820, undertook, at my request, the inquiry, and his careful examination has confirmed its accuracy.

I have the honour to be, your humble servant,

T. R. Underwood.

Article X.

Analysis of the Argillaceous Iron Ore.

By R. Phillips, FRS. &c.

It is well known that the greater part of the immense quantity of iron yielded by the mines of this kingdom is obtained from what is called the argillaceous iron ore. A specimen which I analyzed was of that variety, which is called at Low Moor Iron Works near Bradford, Yorkshire, *Black Iron Stone.*

Its colour, as its name imports, is nearly black; its sp. gr.

3·055, it yields easily to the knife, and becomes magnetic when heated by the blowpipe.

a. 100 grains of this ore reduced to powder and moderately dried on a sand bath, lost one grain, which was evidently mere hygrometric moisture.

b. As it effervesces strongly when put into an acid, 100 grains were put into a vial containing sulphuric acid, the weight of which and its contents were together noted. After the effervescence was over, which took place slowly, it was found that 29·3 grains of carbonic acid were given out. The iron was evidently in the state of protoxide, for crystals of protosulphate of iron were obtained. Indeed no percarbonate of iron probably exists, or at any rate none has been described, and it cannot be formed artificially.

c. 200 grains of the ore were treated with muriatic acid; nitric acid was added to convert the protoxide of iron into peroxide. The solution was decomposed by ammonia, and the peroxide of iron precipitated, being washed, dried, and ignited, weighed 97·5 grains. On repeating the experiment the mean result was 96·15 = 48·075 per cent. The iron in the ore exists as already mentioned in the state of protoxide, and as 40 peroxide are equivalent to 36 protoxide, 48·075 are equal to 43·26 protoxide of iron, which is the quantity contained in 100 grains of the ore.

d. The residuum insoluble in muriatic acid was of a dark colour, and after being moderately dried, it was heated to redness in a platina crucible; by this it became perfectly white, and lost 5·33 grains, which is the weight of the carbonaceous matter. The insoluble residuum, consisting of silica and alumina, gave a mean of 18·12 per cent.

e. The ammoniacal solution, after the separation of the iron, was treated with carbonate of ammonia; by this a quantity of carbonate of lime was thrown down; it weighed in one experiment 7, and in the other 6·5 grains, giving a mean of 3·74 of lime, which of course existed in the ore in the state of carbonate.

f. The ammoniacal solution, evaporated so as to expel all excess of ammonia, was treated with prussiate of potash; a precipitate of a light pinkish hue was obtained, but the quantity was too small to allow of determining the quantity of oxide of manganese which it indicated.

It will then appear that the ore, usually, but improperly called argillaceous iron ore, is in fact a carbonate of iron, consisting of

a	Moisture	1·0
b	Carbonic acid	29·3
c	Protoxide of iron	43·26
d	Carbonaceous matter	5·33
	Silica and alumina	18·12
e	Lime	3·74
		100·75

As an atom of carbonic acid is represented by 22, and one of protoxide of iron by 36, the 43·26 of protoxide which the ore contains must be combined with nearly 26·4 of the 29·3 of carbonic acid, leaving 2·9 to combine with 3·74 of lime, a quantity of carbonic acid which exceeds but little the requisite proportion, and which will not account for the excess in the analysis. Although in this one from Yorkshire the quantity of oxide of manganese is extremely small, there are some of the ores in Wales which contain nearly 10 per cent. of it.

ARTICLE XI.

ANALYSES OF BOOKS.

Pharmacopœia Collegii Regalis Medicorum Londinensis, 1824.

I HAVE been more than once called upon by what I conceived to be the imperfect attempts of the College of Physicians towards improving the Pharmacopœia, to speak in terms of almost unqualified disapprobation of their proceedings; and although it is impossible to deny that many of the errors committed in the edition of 1809 have been rectified on the present occasion; yet it appears to me that the performance is of a much less perfect description than the public have a right to demand; and I shall now briefly state the cases in which, I think, errors have been committed or suffered to remain uncorrected, premising, however, that the College appear to have exercised a sound discretion in refusing to admit many of the novelties of the day into their Pharmacopœia.

No alteration has been made by the College with respect to the arrangement of the Pharmacopœia, and if that which is adopted be not the most scientific or perfect that might have been selected, it does not, that I am aware of, include any very marked inconsistence, and it was, therefore, most prudent to suffer it to remain without any alteration.

The College have introduced into the Materia Medica, Acidum Aceticum fortius, which is explained to be Acidum Aceticum é ligno destillatum; its specific gravity is stated to be 1·046, and 100 grains of it are said to decompose 87 grains of crystallized subcarbonate of soda. The introduction of a pure acid is an improvement. I have not examined any of exactly this strength, but admitting that 100 grs. decompose 87 of crystallized subcarbonate of soda, it must be very nearly six times as strong as good distilled vinegar, now called acidum aceticum dilutum. When properly prepared by the manufacturer, the stronger acetic acid procured by the decomposition of wood is much pleasanter than that derived from the mere distillation of vinegar;

and I think the College might advantageously have omitted the latter, using the former in all cases instead of it. Added to which acetic acid is scarcely a proper appellation for an acid containing so much mucilage, that acetates are with difficulty formed by using it.

In the last Pharmacopœia directions were introduced for the preparation of citric acid; these are still continued, although the acid is now also introduced into the materia medica; in the present Pharmacopœia, a formula is also given for preparing tartaric acid, but by what I presume to be an oversight this acid is not included in the Materia Medica, as it ought to have been for the same reasons that induced the placing of citric acid under that head. With respect to the tartaric acid, it may also be observed, that one-half of that which the bitartrate of potash contains is lost. The method directed is to saturate the excess of acid in the bitartrate by causing it to decompose carbonate of lime, and then the tartrate of lime formed is to be treated with sulphuric acid. As far as it goes this is well, but no notice whatever is taken of the remaining tartrate of potash, which the College should have directed either to be crystallized; or to be decomposed in the well-known manner by muriate of lime, this being a refuse product of more than one pharmaceutic preparation. With regard to benzoic acid, the College have adopted an improvement by restoring the process by sublimation, which they rejected from the Pharmacopœia in 1809. But the medicine is too unimportant to require any particular notice. In preparing muriatic acid, the College direct, as before, that its specific gravity should be 1·160; but they have now given the saturating power of crystallized carbonate of soda as a measure of its strength, instead of that of carbonate of lime, as formerly. I found that the acid procured by the College process had a specific gravity of 1·1645, and as 100 parts of it decomposed almost exactly 44 of carbonate of lime, they would saturate the equivalent quantity of crystallized carbonate of soda, or 134 parts. The College say 124, and this, therefore, is probably almost correct with respect to acid of specific gravity 1·160 as directed.

No alteration has been made in the process for preparing nitric acid; but with regard to the standard for ascertaining its saturating power, that, as in the case of muriatic acid, is changed from carbonate of lime to subcarbonate of soda; and although it is stated with great precision that 100 grains saturate 212 of crystallized subcarbonate of soda, (the quantity by calculation being only half a grain more,) I am yet of opinion that the method of using carbonate of lime is the best, for reasons which Dr. Wollaston has stated; the fact, however, is, that neither plan is at all needful when the specific gravity is stated. With, however, the near approach to accuracy which the College have now made, there can be no objection to the statement. I cannot conceive why the College persist in ordering that the nitric acid obtained by the first part of the process which they direct, should be redistilled with an additional quantity of nitre. The acid is as pure as possible, and has the solvent power and specific gravity which they attribute to the rectified acid; and by omitting the redistillation, time, trouble, expense, waste, and a disagreeable operation, are avoided, without losing even the shadow of an advantage.

The directions for preparing liquor ammoniæ, as far as the proportions of the ingredients are concerned, remain unchanged, and so also does the quantity of the fluid to be distilled from them; but it is now directed that the temperature of the receiver should not exceed 50 degrees. Now one of two things must happen; the strength of the product is either increased by this alteration, or it is not. If it be unattended with advantage, the only remark required is obvious:—why make the alteration? If, on the other hand, the strength of the solution is increased as one might expect, why is its specific gravity, and consequently its strength, stated to be similar to what it was in the last Pharmacopœia? The specific gravity was there fixed at 0·960, and it remains the same, although we are now directed to keep the receiver cooled to 50° for the obvious purpose of rendering it more powerful.

The method of preparing potassæ acetas is altered, but I much question whether any improvement has been introduced, except that of using pure acetic acid instead of distilled vinegar; by this the inconveniences arising from the mucilage which the latter contained will certainly be obviated, and I have no doubt but a pure and white salt is at once procurable. In the late Pharmacopœia, the directions were to boil the solution of acetate of potash to dryness, but now the solution is to be evaporated until a pellicle forms, and this being removed is to be preserved and dried, as acetate of potash. I have not tried this process, but if practicable, it must, I think, be tedious, and unattended with any advantage over the method of evaporation to dryness.

The next alteration which I shall notice is an unquestionable improvement—that of procuring carbonic acid from the decomposition of carbonate of lime for the purpose of preparing carbonate or rather bicarbonate of potash, instead of obtaining it as in the last Pharmacopœia, (and even then in too small quantity,) from the decomposition of subcarbonate, or rather sesquicarbonate of ammonia. But here my commendation must end; for by a very obvious oversight, the College have directed the solution of carbonate, (bicarbonate) of potash, to be evaporated for the formation of crystals, when there is not actually more than about one-fifth of the water present necessary for their solution. Indeed on account of the very strong solution of the subcarbonate (carbonate) of potash which the College direct for conversion into bicarbonate, I have but little doubt that the tubes which, on common occasions, would be used for conveying the gas, would soon be choked with crystals, and the whole apparatus would probably be blown to atoms.

With respect to sulphate of potash, the College would have acted economically in imitating the directions of the Edinburgh Pharmacopœia, by saturating the excess of acid of the bisulphate, with lime instead of potash; by this the waste would have been avoided, of using a salt of greater value to obtain one of less. But as potassæ sulphas is now also in the Materia Medica, the chemist may avail himself of those economical modes of operating which the College have too frequently neglected, and that without any attendant advantage. The same remarks will also apply to the preparation of sulphate of soda.

The directions for preparing that important medicine, tartarized antimony, are most exceedingly and unequivocally improved: there is indeed nothing new in the process, but it is simple and effectual; whereas the method which has given place to it was the worst, considering that it really was practicable, that ever was devised. The College have now directed the use of the glass of antimony, but I think that the proportion ordered is rather too small to convert the whole of the tartar into tartarized antimony.

The preparation which I shall next notice is the vinum antimonii tartarizati. In those preparations in which wine was formerly employed, the College have now directed very dilute spirit to be used, still retaining, however, with unquestionable impropriety, the appellation of *vinum*. In the present instance, I am not aware that the change has been either beneficial or otherwise; but I shall presently notice a preparation in which the alteration has been decidedly hurtful, because the College, without seeming to have been aware of the fact, have much weakened the medicine. Added to this, dilute spirit of very different degrees of strength is now used instead of wine, which must, I think, have been more uniform in its power.

Among metallic preparations a place has been given to subnitrate of bismuth. The process, which is extremely simple, I have not examined; but even if the metal or acid should either of them be in excess, the precipitate will at any rate be obtained without difficulty.

With respect to the preparations of iron, there have been some alterations which are to be considered as amendments; but I am apprehensive that the good which has been done is more than counterbalanced by the omission of improvements, or the commission of errors.

Ferrum ammoniatum is so weak a preparation as, perhaps, scarcely to be worthy of notice; the process for obtaining it has been altered; it could scarcely have been rendered worse, and yet I do not think it has been improved. In the late Pharmacopœia this preparation was directed to be formed by subliming a mixture of muriate of ammonia and subcarbonate of iron; the consequence of which was, that until the carbonic had combined with a considerable portion of the ammonia of the muriate, and formed carbonate of ammonia, no ferrum ammoniatum could be obtained, and then only after considerable waste.

Instead of directing a mixture of muriate of ammonia and carbonate of iron to be subjected to sublimation, the College have now ordered the carbonate of iron to be mixed with muriatic acid, and the substance remaining after evaporation to dryness, to be mixed with muriate of ammonia, and then submited to sublimation. It so happens, however, that the muriatic acid dissolves scarcely more than one-third of the subcarbonate of iron; so that waste appears to be incurred. Indeed the better and more obvious process for procuring this medicine is simply to boil down a mixed solution of muriate of ammonia and permuriate of iron to dryness. By this a preparation of uniform strength and unquestionable efficacy would be obtained; and those practitioners would be gratified who are of opinion that muriate of ammonia is a useful adjunct to permuriate of iron.

In preparing the ferri subcarbonas, the College still continue the wasteful process of directing nearly one-half more sulphate of iron to be used than the subcarbonate of soda is capable of decomposing. No excuse can be offered for this, since it had been pointed out for correction. Six parts of subcarbonate of soda only are directed to be used with eight parts of sulphate of iron, of which they can decompose only about 5·4 parts. It is indeed true that sulphate of iron is a cheap material, but the chemist should have the opportunity of using it to the best advantage, and errors of this description furnish those who are inclined to neglect the directions of the College in other respects with too plausible a pretext for so doing.

Ferrum tartarizatum, the preparation next to be noticed, has been much improved by the alterations which the College have introduced. Instead of ordering the whole mass to be dried, and which contained a considerable quantity of iron unacted upon, the soluble portion only is directed to be used, and the aqueous solution of it evaporated to dryness constitutes ferrum tartarizatum. There is, however, one part of the process now introduced which is perfectly useless—I allude to the directions for boiling the metal in the solution of tartarized iron towards the end of the process. No effect can surely be hoped to be produced by ebullition for fifteen minutes, after twenty days' exposure to the air have ceased to cause any action.

Vinum ferri still retains its title, although deprived of any just claim to it by the substitution of dilute spirit for wine; I was at one time apprehensive indeed that it would contain neither wine nor iron. The directions for preparing it are to form tartarized iron with considerable excess of tartar; this being probably intended to supply the slight acidity which wine usually possenses, and to which its solvent power is owing. The quantity of iron directed to be used appears to me to be almost exactly such as, if it were all dissolved, would render the medicine of such a degree of strength, as I found it actually to possess when prepared with sherry wine; namely, 22 grains of peroxide of iron in a pint; the College order 60 grains of iron to be converted into tartarized iron, and the compound formed to be dissolved in 60 ounces of dilute spirit; and as 60 grains of iron are convertible into about 85 grains of peroxide, and as 60 ounces would contain this quantity, a pint, or 16 ounces, would hold about 22·6 grains in solution.

It happens, however, unfortunately, that three causes conspire to prevent this medicine from possessing the strength of the former; first, the whole of the iron is not dissolved by the supertartrate of potash; secondly, a part of that which is dissolved is rendered insoluble by the process of drying; and lastly, a portion of what the water dissolves is immediately precipitated by the spirit, and the quantity is so very considerable that the vinum ferri now contains only 16 grains of peroxide in a pint instead of 22 as formerly. In this preparation, therefore, merely for want of experiment, and by taking that for granted which did not happen to be true, the strength has been reduced to about two-thirds that which it formerly possessed, and no notice whatever is of course taken of the change.

In preparing calomel, the College have introduced what is certainly a very considerable improvement; namely, forming it by one sublimation, instead of first preparing corrosive sublimate, and then triturating that with an additional portion of mercury, and subliming again; the present process consists in mixing bipersulphate of mercury with mercury and common salt, and subjecting the mixture to sublimation; the sublimed calomel is then to be reduced to powder, and washed with a solution of muriate of ammonia, in order, I presume, the more readily to dissolve any corrosive sublimate which may have been formed; this, perhaps, might have been spared, for corrosive sublimate is sufficiently soluble in water to admit of its separation from calomel during the process of elutriation. If, however, no corrosive sublimate be actually found with the calomel, the manufacturer may use the same solution of muriate of ammonia repeatedly, and if, on the other hand, any be dissolved, it may be converted into hydrargyrum præcipitatum album.

In comparing, however, the processes for procuring corrosive sublimate and calomel, a strange disregard of proportions has occurred; in order to convert 24 parts of mercury into corrosive sublimate, the College direct the use of 30 parts of sulphuric acid and 48 of common salt; while in preparing calomel, 48 parts of mercury are to be treated with 30 ounces of sulphuric acid, and only 18 of common salt. Now it must be granted upon any theory that 24 parts of mercury will require exactly as much common salt to convert them into corrosive sublimate as to convert twice the weight of mercury into calomel, and yet 48 parts of common salt are ordered for the production of an effect in one case, while an equal effect is to be produced in the other by 18 parts; the fact is, that the 48 parts are much in excess, as I pointed out long since; 49 parts of sulphuric acid cannot decompose more than 60 parts of common salt; but in preparing corrosive sublimate, after about 12 parts of the 30 of sulphuric acid directed to be boiled with the mercury are decomposed by oxidating it, the remaining 18 must be supposed capable of decomposing 48 of common salt; it is, therefore, evident that about 26 out of 48 of salt are wasted. Why the quantity was not diminished as to be more in agreement with the directions for preparing calomel, I cannot discover.

The preparations of lead do not call for any particular observation, excepting, that as the sulphate of copper, soda, and potash, are introduced into the materia medica, no reason can possibly exist why the acetate of lead should not also have been; it is perfectly well prepared for the purposes of the arts, and the acetic acid of which it is made not being subject to any duty, it may be purchased at so much cheaper a rate than that at which it can be prepared, the formula is therefore useless; it may also be observed, that the acidum aceticum fortius diluted with water would have answered the purpose of making the liquor plumbi subacetatis.

Oxide of zinc formerly prepared by combustion is now directed to be formed by decomposing the sulphate of zinc with ammonia. I have not examined the preparation; there is a slight difficulty attending the use of the caustic alkalies in precipitating oxide of zinc, which is, that if accidentally added in excess, they redissolve the precipitate at first formed. On this account I prefer carbonate of soda or potash, which I recommended some years since for this purpose. The advantage of the new mode of preparation is, I think, considerable, as it prevents the presence of any minute portions of metallic zinc which were apt to render the ointment of zinc gritty.

It appears to me that the College ought to have been more consistent in their directions. I have already shown some instances of their deficiency in this respect, and the spirits of ammonia afford additional proofs of want of attention. In the Pharmacopœia of 1787, the spiritus ammoniæ and spiritus ammoniæ aromaticus are similar in strength, varying only in the aromatics which the latter contained; but in the present Pharmacopœia, and indeed in the last, as I pointed out, the proportions of spirit, muriate of ammonia, and carbonate of potash, are extremely different. In preparing the spiritus ammoniæ, 32 parts of muriate of ammonia are directed to be decomposed by 48 of subcarbonate of potash, a quantity which is too small by about three-fourths of a part; in making the spiritus ammoniæ aromaticus, the proportions are 82 of muriate and 38·4 parts of subcarbonate, but as 48·75 of the latter are requisite, it is evident that 6·8 parts of the former escape decomposition, and are wasted. The spirit of the simple preparation is rectified, whereas that of the aromatic spirit of ammonia consists of two parts of rectified and one of water. Now this quantity of water, although useless, would have been of little consequence in the simple spirit, because in using it for the spiritus ammoniæ fœtidus (the only purpose to which it is applied), the water would remain in the retort; but it is of importance that the aromatic spirit should contain as little water as possible that in preparing the tincture of guaiacum, the solution of the resin may not be prevented.

With regard also to the proportion of ammonia in these preparations, it will be seen that the difference is enormous: 24 fluid ounces of the simple spirit contain the carbonate of ammonia obtained by decomposing nearly 82 drachms of the muriate, while an equal quantity of the aromatic spirit contains the carbonate procured by decomposing rather less than 8 drachms of the muriate of ammonia. It is indeed true that 10 drachms are directed to be employed; but by the obvious and unaccountable inconsistency of using only 12 instead of 15 drachms of subcarbonate of potash, one-fifth of the muriate of ammonia, and even rather more, escapes decomposition as already noticed.

The method of preparing the spiritus ammoniæ fœtidus is extremely wasteful, and more so even than that of spiritus ammoniæ; of the 28 parts of spirit employed in the latter preparation, 4 are wasted; and in using the remaining 24 to prepare the fetid spirit, 6 more are thrown away; consequently out of the 28 parts of rectified spirit originally made use of, only 18 are eventually employed.

In closing my remarks upon this work, it must be fairly admitted, especially with respect to tartarized antimony, calomel, and some other preparations, that the College have removed much of the objectionable matter of the Pharmacopœia of 1809, and of the *editio altera* which followed it; but there is yet, for the reasons which I have now stated, much that appears to require a correcting hand. I will only add, that it is greatly to be lamented that a national Pharmacopœia should not be formed by the union of the Colleges, by which greater facilities would be afforded to physicians in the various parts of the kingdom, in describing their modes of treating different diseases.—[*Edit.*]

Article XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

April 8 (*addendum*)—Sir Francis Shuckburgh, Bart. was admitted a Fellow of the Society.

April 29.—E. H. Lushington, Esq. and the Rev. Dr. E. Maltby, were admitted Fellows of the Society; and the name of Woodbine Parish, Esq. ordered to be inserted in its printed lists, he being unable to attend for admission. Among the presents received was a portrait of Mr. Smeaton, the celebrated Engineer, bequeathed to the Society by his daughter, Mrs. Dixon.

A Letter from Dr. Tiarks to Dr. T. L. Young, For. Sec. RS., as Secretary of the Board of Longitude, was read: it related chiefly to observations made on the longitude of various places in England in 1822 and 1823.

May 6.—Lieut. Henry Forster, RN. was elected into the Society, and being on the eve of departure in the new expedition under Capt. Parry, was immediately admitted a Fellow.

The reading was commenced of a paper "On Univalves; by Charles Collier, Esq. Staff Surgeon:" communicated by Sir James Mac Gregor, Bart. FRS.

May 13—The Earl of Orford, the Rev. Dr. Goodenough, Philip Barker Webb, Esq. and John Gage, Esq. were respectively admitted Fellows of the Society.

The reading of Mr. Collier's paper was concluded: and a paper was read, "On the Variation of the Rates of Chronometers with the Density of the Atmosphere; by George Harvey, Esq. FRSE:" communicated by Davies Gilbert, Esq. VPRS.*

May 20.—The Rev. Baden Powell was admitted a Fellow of the Society.

A Letter from Professor Berzelius to the President was read, in which he describes the results of various chemical researches in which he has recently been engaged; and several memoirs on which accompanied the letter. The first memoir relates to the analysis of the Carlsbad waters; which we have already noticed in the *Annals*, N. S. vol. v. p. 396. The next contains researches on the combinations of acetic acid with oxide of copper: in this, Prof. Berzelius says, he has pointed out the errors in the analyses of these salts into which Mr. Phillips and other chemists have fallen. The fourth memoir relates to experiments on the compounds of oxide of uranium, in which

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* See our last number, p. 392.

Prof. B. has confirmed the results obtained by M. Arfwedson, and also Mr. Phillips's discovery of phosphoric acid in the uranite. He has examined the uranite of Autun, and also that of Cornwall: finding the former to be a phosphate of uranium and lime, and the latter a phosphate of uranium and copper; the number of atoms of water being the same in both. The third contains an examination of a mineral in an old collection at Stockholm, labelled 'from Mendip, near Churchhill, in Somersetshire.' This substance consists of 1 atom of chloride of lead, and 2 atoms of oxide of lead, and also contains carbonate and molybdate of lead. It is distinct from the murio-carbonate of Matlock. The fifth memoir relates to the combinations of fluoric acid. A portion of this memoir now printing describes a method by which the author has succeeded in obtaining the base of silica in an insulated state. It consists in acting by potassium on dry silicated fluate of potash, by which means a mixture of various substances is obtained, which yields hydroguret of silicon by being well washed with water: and when that substance is heated in a crucible the hydrogen is burned off, and the silicon obtained pure. Prof. B. then proceeds to give the results of various experiments upon this substance; among which are the following. It is obtained in various states of aggregation, and its combustibility varies accordingly, it much resembling carbon in this respect: as usually obtained it is combustible when ignited in atmospheric air and in oxygen gas; but in its dentsest state it may become incandescent in the air without burning. It is very difficult to effect its complete combustion: 200 parts of silicon unite to 208 of oxygen to become silica. It will not burn when heated with nitre, but is brought into combustion by carbonate of potash; a curious circumstance which the author attributes to certain relations of affinities. Silicon burns when ignited in chlorine, forming with it a transparent colourless fluid, having the smell of cyanogen. It is combustible in vapour of sulphur, producing a grey sulphuret, but cannot in this case be completely burned.

Prof. B. next describes the results of the same mode of decomposition as applied to ittria, glucina, and zirconia; giving the chemical habitudes of zirconium, which can be obtained in larger quantities than the bases of the former earths. He then states that he has used the term *fluate* instead of *fluoride* throughout this letter, not because he thinks the President's ingenious theory on the subject less probable than his own (though he has not been able, by his own experiments, to determine which is the true one); but because, as he was writing in a language foreign to him, he wished to employ the plainest terms: and concludes by requesting Sir Humphry to lay the above results before the Royal Society.

The reading was commenced, of a paper "On some new phænomena effected by magnetic influence, by J. H. Abrahams," of Sheffield: communicated by Mr. Tooke, FRS.

GEOLOGICAL SOCIETY.

March 5.—The paper entitled "Outline of the Geology of the South of Russia," by the Honourable William T. H. Fox, Strangways, MGS. was concluded.

The term Steppe is applied to vast tracts of country in the E. and SE. of Europe. It is neither a heath, nor a moor, nor a down; *wold* would give the best idea of it in English, and it is given by the Russians to any waste land which is neither mountainous nor wooded. The Russian Steppes are bounded on the west by the Carpathian chain of Transylvania and the Banat of Temesvar; on the S. by Mount Hæmus, the Tauric Chersonese, and Caucasus; on the E. by the Oural mountains to beyond the Caspian Sea and the sea of Aral; vaguely to the N. by a line from the mouth of the Kama to the Dniesters on the frontiers of Podolia and Kherson. Their length is about 2000 miles, breadth 900. The soil is similar throughout; the geological structure very different.

A trough or basin stretching across from Perecop to the Caspian, and thence beyond the sea of Aral, forms a natural division of the Steppe into the N. and S. High Steppe; this trough or basin Pallas and others well describe as the low sandy saline steppe; the two former as the high rich calcareous and granitic steppe.

The Northern High Steppe admits of five divisions:

1. Steppe of red marl, salt, and gypsum, lying on both sides the Volga above the reach of Samara. 2. Steppe of Saratof and the middle Volga, from Samara to Tzaritzin; its northern part consists of the white central limestone, its southern of sandstone which connects it with the steppe of the Don. 3. Northern calcareous steppe of the Don is composed of sandstone to between Cherkask and the mouth of the Donetz; here commences an immense tract of a peculiar modern shelly limestone; the steppe limestone probably extends across the Ukraine, and is connected with the calc. gross. of Volhynia and Gallicia. 4. S. and SE. of this occurs the primitive or granitic steppe, a singular instance of a flat tabular granitic country connected, according to Pallas, with the primitive range of the Carpathians, passing the Dniester at Doubosar, and traversing Moldavia. 5. Middle calcareous steppe, of steppe limestone separated by a sandstone from the preceding; this is a prodigious mass extending throughout Wallachia, Bessarabia, the south of Moldavia, and Government of Kherson. The trough or basin before alluded to forms the steppe of the old sea, which involves the singular problem of the connexion and extension of the Caspian and Black Seas. To the south of this lies the southern calcareous steppe, comprehending the Crimea, and stretching to the foot of Caucasus, is composed of steppe limestone resting on calc. gross. The high steppes, from the occurrence of marine plants and other causes, have been supposed to have once

formed a vast sea; but their height, in some places 700 feet above the Black Sea, and 1000 feet above the Caspian, precludes the possibility of this.

The author, after enumerating and describing the series of the above-mentioned beds, and their accompanying fossils, concludes with remarks on the probable extension of the Caspian Sea, and the sea of Aral, and their connexion with the Black Sea by means of the low steppe.

A letter from Mrs. Maria Graham to Henry Warburton, Esq. VPGS. was read, giving an account of the effects of the Earthquakes which visited the coast of Chili in 1822 and 1823.

The first shock by which the towns of Valparaiso, Melipilla and Quillota were nearly destroyed, was felt at a quarter past 11 o'clock on the evening of Tuesday the 19th of November, 1822; and from this time continual shocks were felt daily until the 18th of January, when the authoress ceased to reside in Chili. These shocks are said not to have terminated wholly so late as September last. The sensation experienced during the more violent shocks was that of the earth being suddenly heaved up in a direction from N. to S. and then falling down again, a transverse motion being now and then felt. On the 19th of November a general tremour was felt, and a sound heard like that of vapour bursting out, similar to the tremour and sound which the authoress observed while standing on the cone of Vesuvius during the jets of fire at the eruption of 1818. In all the alluvial valleys in the neighbourhood of Quintero, 30 miles N. of Valparaiso, quantities of water and sand were forced up, which covered the plain of Vina a la Mar with cones or hillocks four feet high.

The promontory of Quintero, consisting of granite covered by sandy soil, was cracked in various directions down to the sea; and the cracks occasioned by the earthquake in the granite on the beach were parallel to the more ancient rents in the same rock.

On the morning of the 20th, after the first earthquake the whole line of coast from N. to S. to the distance of 100 miles was found to have been raised out of the sea; the elevation at Quintero being about four feet, that at Valparaiso about three feet, beds of oysters and muscles, adhering to the rock on which they grew, being seen lying dry on the beach.

Similar lines of beach with shells are found parallel to the coast to the height of 50 feet above the sea, which probably have been occasioned by earthquakes which have in former years visited Chili.

The earthquake of the 19th was felt along the coast to the distance of 1400 miles at least.

March 19, 1824.—A paper entitled "Sketch of the Geology of New South Wales and Van Diemen's Land," by the Rev. T. H. Scott, was read in part.

April 2.—The paper entitled "Sketch of the Geology of New South Wales and Van Diemen's Land," by the Rev. T. H. Scott, was concluded.

The coast of New Holland, from Cape Howe to Port Stephens, including Botany Bay, Port Jackson, &c, as examined by Mr. Scott, consists of an uninterrupted series of the coal measures. At Illasvarro, or the five islands, a seam of coal is found at the surface. Between Broken-Bay and Port Hunter, a horizontal seam of coal is bared by the action of the sea on the cliffs. Very good coal is worked at Newcastle on Hunter's river, 37 yards from the surface, 3-feet 1-inch thick; it is intersected by trap-dykes in some places; and vegetable remains of a large leaved fern, thought by the people to be an Euculyptus, are picked up at the base of the Cliff. Limestone alternates with the sandstone, and iron ore occurs. The wells at Sydney, being not more than 30 feet deep, the water is not good; one well, sunk 82 feet to a great mass of sandstone, gives excellent water. From Paramatta the coal measures continue, and are broken by trap dykes at the Nepean to Emuford; where the ascent of the blue mountains commences, near the summit of which, the coal measures rest on the old red sandstone. The escarpment of this rock on the east side presents the aspect of a perpendicular wall, at the top of which the old red Sandstone is found in contact with primitive rocks: these occur in the vale of Cleuyd and Clareneers hilly range; where the Macquarrie rises, and, after a north-east course of 300 miles, terminates in a vast swamp. Returning westward, porphyritic rocks and clay slate accompany the primitive rocks near Bathurst and the Sidmouth range, to lake George and the Cookbundoon river, which continue to the cow pastures, where the coal measures of the colony again appear.

The geology of the Island of Van Diemen's Land is conformable to that of the continent of New Holland. Both Hobart-town and George-town are upon the coal formation. Between the former and Elizabeth-town, a limestone full of shells is found, probably of the oolite series, and the same rock occurs near George-town on an island in the Tamar. In the middle of the island at Bagdad, a rock, which answers to the description of the millstone grit, and salt, are found on the river Macquarrie. To the east and the west of the inhabited tract between the two towns, high mountains and elevated primitive ridges are alone discoverable, so that the island probably contains little other fertile soil to tempt future emigration when this space shall have peopled, which is not the case in New South Wales.

A letter was read on a section obtained in sinking a well at Streatham, by Mr. J. S. Yeats, communicated by G. H. Brown, Esq.

A well having been sunk at Streatham to the depth of 285 feet, the greatest depth which has been pierced in that part of the country, the following section was exhibited. From the depth of 2 feet to 29 feet, stiff reddish brown clay; from thence to 35 feet, clays with septaria; from thence to the depth of 180 feet, blue clay, in which, in from 70 to 100 feet, were found various shells and fragments of bituminous wood with iron pyrites; from 200 feet to the depth of 230 feet, blue clay, sometimes sandy, in which numerous shells and bituminous wood occurred; at 230 feet, round black pebbles of flint like those of Blackheath were found, this appearing to be the point of junction between the London and plastic clays; next a bed of sand, and afterwards various coloured clays, were pierced; at the depth of 270 feet, and continuing to 285 feet, sand and sandy clays occur, the greater part of which is full of green earth exactly resembling that of the oyster bed at Reading. The paper was accompanied by specimens of each of these strata.

A letter was read from Alexander Gordon, Esq. to D. Gordon, Esq. of Abergeldie, describing three successive forests of fir imbedded in a peat moss; accompanied by specimens.

The moss of Auldguissack in Aberdeenshire, Scotland, presents an inclined plane of rather uneven surface, and varies in depth from 18 inches to 10 feet from the lower part of the hill to the river.

Upon digging up the ground in two different parts of the moss, large roots of Scotch fir trees were found about one foot below the ordinary average level of the moss. Below the bottoms of these roots there is a stratum of about a foot and a half of moss below which other roots or trunks appeared, and on digging still further down (about six or seven feet below the ordinary level of the moss), a third set of roots and truncated stems of trees were discovered.

It appeared to Mr. Gordon impossible that these roots could have supported different trees, all growing at the same time, for the distinct ramifications of these (horizontally like Scotch firs at the present day), are bedded in moss perpendicularly above each other.

April 23.—A paper was read, entitled "Some Observations on the Lakes of Canada, their Shores, Communications, &c. by Lieut. Portlock, RE."

In this memoir the author describes the various nature of the shore, of Lakes Huron, Michigan, Erie, and the other lakes of Canada, and annexes a plan, in which a tabular view is presented of the comparative level of these lakes and their communications with each other. At the falls of Niagara, he observes, the upper stratum is a firm compact limestone resting on strata of a very schistose nature. It is not by erosion of the surface that the falls are made to recede, but the waters, after falling 150 feet, strike the bottom, and are reduced to foam; they are then driven up into the air far above the rock whence they had descended: this penetrating foam acts on the lower argillaceous strata, till the overhanging rock is undermined. Lieut. Portlock remarks that there has been a gradual fall in the level of the lakes at Canada. He also offers some considerations on the proximity of the sources of several rivers which flow in opposite directions.

May 7.—A paper on the Geology of the Ponza Islands in the Mediterranean, by G. P. Scrope, Esq., MGS. was read in part.

A letter was read from Thomas Borfield, Esq., MGS. accompanied by a collection of bones and horns of the Deer, and bones of Man and other animals, found in a cleft of the rock at a quarry at Hincks' Bay (near the Old Park iron-works) in the parish of Dawley and county of Salop. Their adhesion, when applied to the tongue showed that the animal gelatine was nearly gone, which does not take place till after a long period of inhumation.

METEOROLOGICAL SOCIETY.

Feb. 11.—Dr. Burney communicated the results of a Meteorological Journal, for January 1824, kept at his observatory at Gosport, Hants.

A note was read, on some curious effects of the Radiation of Heat; by Luke Howard, Esq. FRS. and M. Met. Soc.

The reading was commenced of a "Memoir on the Variations of the Reflective, Refractive, and Dispersive Powers of the Atmosphere, &c.; by T. Forster, MB. FLS. and M. Met. Soc."

March 10.—The reading of Dr. Forster's memoir was concluded. It relates to certain branches of the subject of atmospheric refraction, belonging to the province of meteorology, which the author states to have been particularly neglected: these are, the variation in the refractive, dispersive, and reflective powers of the atmosphere, resulting from the diffusion therein of different modifications of cloud, which are themselves affected by local circumstances, and which vary greatly at different times; and the effects of that variation on the colour of the light transmitted by the planets and fixed stars, and on the declination of the latter. After some general remarks on reflection, refraction, and prismatic dispersion, the author proceeds to consider the subjects just mentioned in three sections. In the first, 'On the variation in the refractive power of the atmosphere at different times of the night and day, and on different occasions and seasons,' he ascribes that variation, principally, to the quantity and nature of the aqueous vapour diffused in the air; and he supports this opinion by various observations on the planets and stars, made at different times and seasons. In observing the planets and brightest stars through prismatic glasses, he found that the spectrum was less oblongated, whilst the red colour was more distinctly apparent, at the period of the vapour point, than at almost any other time of the same nights. On other occasions, at the same period of evening, the violet and in general the colours of the most refrangible rays were most conspicuous, and the spectrum was more oblongated than ordinarily. Dr. F. at length ascertained that the greater prevalence of the red in the spectrum, uniformly accompanied that state of the atmosphere, when the *cirrostratus* diffused itself, after sun-set; whilst the more oblongated spectrum, with the violet, and most refrangible colours, attended an atmosphere, in which the condensing vapours assumed the form of *stratus*. He infers from these and other observations, that the changes in the qualities of the diffused vapour in the air must produce great variation in the atmospherical refraction. In the second section of his memoir, he suggests that local circumstances may produce great variation in the mean refractive power of the atmosphere at different places; and that the discordances in the places assigned to the fixed stars in different catalogues of them may have resulted from such variation. In the third section, entitled 'Of Varieties in the Composition and Nature of the Light of different Stars, considered as still further varying the Effects of Atmospherical Refraction, Reflection, and Dispersion,' Dr. Forster details a number of minute observations upon those varieties; proceeds to inquire into their causes; and concludes with an account of some experiments on the decomposition of the light of the moon, the planets, and certain fixed stars.

A paper by Dr. Forster was also read, "On the great depression of Temperature which occurred in January, 1820."

"The remarkable depression of temperature, related in this paper, took place at Hartfield, in Sussex, to the neighbourhood of which place it appeared to be confined, during the period between sun-set on January 14, and midnight of January 15, 1820. At 10 p. m. on the 14th an out-door Fahrenheit's thermometer exposed to the NE. was at zero, and at 11 o'clock it indicated − 5°. Sometime between the hours of 1 and 8 a. m. on the 15th, it sunk to − 10°, as shewn by a Six's Thermometer. It thence gradually rose, until at midnight on the 15th, it attained the elevation of + 23. A thermometer exposed to the NW. indicated 1° higher in each observation. During this period of excessive cold, the air was calm and clear, a few ill-defined cumuli only were seen on the 15th; the snow which had fallen on the 13th lay on the ground. Dr. F. received only one notice of a distant observation, made at Canterbury, where a Thermometer in-doors indicated 0°; which was also the temperature in-doors at Hatfield, on the morning of the 15th.

Dr. Burney communicated the Results of his Meteorological Journal, for February.

April 14.—A note was read on certain Phænomena of the late Cold Weather, &c.; by Luke Howard, Esq. FRS. M. Met. Soc.; and Dr. Burney communicated the Results of his Journal for March.

May 12.—Dr. Burney communicated the Results of his Journal for April; and the following paper was read:

"An Account of the principal Phænomena of Igneous Meteors which were observed in the Year 1823; forming part of a Review of the Progress of Meteorological Science during that Period: with remarks on the Characters of certain Meteorites." By E. W. Brayley, Jun., ALS. and M. Met. Soc. In this paper the author first describes from various authorities, the Fire-balls which were observed, respectively, on Jan. 26, 1823, at Gosport; on the 23d of May, at Kiel in Denmark; and on the 20th of August, at Ragusa. The latter, being contemporaneous with an earthquake at the same place, gives occasion for an inquiry how far the appearance of Igneous Meteors may be considered as an attendant phænomenon of earthquakes: several meteors of this kind, it is observed, were seen in the province of Cutch, at the time of the extensive earthquake in India in 1819, the most violent motion of which was experienced in that province and its vicinity; and two Fire-balls appeared, one at Zante, and the other at Cephalonia, on the day after the earthquake that desolated the former island in 1820: other instances of this connexion are likewise adduced. Mr. Brayley then proceeds to an examination of the phænomena attending the fall of several Meteorites, at Nobleborough, in the State of Maine, in North America, on the 7th of August last. He next points out a remarkable affinity, in mineralogical characters, subsisting between these meteorites, and those which fell, respectively, at Loutolox in Finland, in 1822, at Jonzac, in France, in 1819, and at Juvenas, in the same country, in 1821; several specimens of the latter being laid before the Society, for the purpose of illustration. This affinity partly consists in the strong resemblance which they all bear to certain products of volcanos; whilst the meteorites of several other descents connect them, by a gradual transition, with those whose characters are more peculiar. From these and other circumstances, in conjunction with that of the frequent presence of Olivine in meteorites, the author infers, that the agencies which give rise to volcanic phænomena, whatever these may be, and however exerted in this case, are probably concerned in the production of Igneous Meteors and the bodies which descend from them. He concludes by recommending the investigation of this curious subject to the members of the Society; promising to lay before them, after the recess, the results of some further researches upon it.

The Society then adjourned, over the Summer recess, to meet again on Wednesday the 13th of October next.

MEDICAL SOCIETY OF LONDON.

The fifty-first Anniversary Meeting of this Society was holden at the London Coffee House, Ludgate Hill, William Shearman, MD. President, in the chair.

The Officers and Council for the ensuing year are as follow: *President*: William Shearman, MD.—*Vice-Presidents*: Henry Clutterbuck, MD.; Henry James Cholmely, MD.; Sir Astley Paston Cooper, Bart. FRS., and Thomas Callaway, Esq.—*Treasurer*: John Andree, Esq.—*Librarian*: David Uwins, MD. —*Secretaries*: T. J. Pettigrew, Esq., and Thomas Callaway, Esq.—*Secretary for Foreign Correspondence*: Robley Dunglison, MD.—The other members of the Council: Drs. Walshman, Hancock, J. G. Smith, Blicke, Blegborough, Hopkinson, Stewart, Ley, Darling, Haslam, Pierce, Cox, and Burne; Messrs. Sutcliffe, Drysdale, Wender, K. Johnson, Dunlap, Kingdon, Ward, Thomas Clarke, Burton Brown, Lake, Ashwell, Edwards, Handey, E. Leese, Skair, Cordell, Bell, Ellerby, Amesbury, T. Bryant, and Burrows.—*Registrar*: James Field, Esq.—The Fellow elected to deliver the annual oration, in March 1825, Eusebius Arthur Lloyd, Esq.

The President informed the meeting that the time allotted for the perusal of the dissertations offered for the Fothergillian medal, during the last year, having been unexpectedly shortened, the Society had not yet adjudicated the prize medal: this however would be done forthwith, and the Medal would be presented to the successful candidate at a Special General Meeting of the Society to be holden on the 3rd of May, at eight o'clock in the evening.

The annual oration was then delivered by Dr. John Gordon Smith, the Ex-Vice President; the subject was, "The Duties and Perplexities of Medical Men as professional Witnesses in Courts of Justice."

A numerous body of Fellows and their friends, amounting in all to 86, afterwards dined together in the great room of the Tavern, the President being in the chair; and the remainder of the day was marked by harmony and conviviality.

Conditions of the Fothergillian Medal.—In conformity with the will of the late Anthony Fothergill, MD. FRS., the Society resolved to give, annually, to the author of the best Essay on a subject proposed by them, a gold medal, value 20 guineas, called the Fothergillian Medal; for which the learned of all countries are invited as candidates.

1. Each dissertation must be delivered to the Registrar, in the Latin or English language, on or before the first day of December.

2. With each dissertation must be delivered a sealed packet, with some motto or device on the outside, and within the author's

name and designation, that the Society may know how to address the successful candidate.

3. No paper in the hand-writing of the author will be received; and if the author of any paper shall either directly or indirectly discover himself to the Committee of papers, or to any member thereof, such paper will be excluded from all competition for the medal.

4. All the dissertations, the successful one excepted, will, if desired, be returned with the sealed packets unopened.

5. The prize medal will be presented to the successful candidate, or his representative, at the Anniversary Meeting of the Society in March 1825.

The subject of the Dissertation to be offered for the Prize Medal for March 1825, is, "The Pathology and Treatment of Periodical Asthma."

Article XIV.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Hydriodate of Potash.*

M. Taddei proposes the following method of preparing this salt: dissolve iodine in spirit of wine, and pour repeatedly a solution of hydrosulphuret of potash into the solution of iodine; the fluid becomes turbid, and changes from the blackest brown to a chesnut colour, and this, diminishing in intensity, gradually becomes flesh coloured, and afterwards milk white. At this period the conversion of the iodine into hydriodic acid is effected; and if the liquid does not become turbid on the addition of a few drops of hydrosulphuret of potash, the operation may be regarded as complete. After standing a few minutes, the precipitated sulphur is to be separated by decantation or by the filter, the mixture is then to be distilled to procure the alcohol employed, and the residuum is to be evaporated to dryness in an open vessel to obtain the hydriodate of potash.—(Giornale di Fisica, etc. 1823.)

II. *Action of Hydrocyanic Acid on Vegetable Life.*

M. Becker has made many experiments, from which it results that hydrocyanic acid, prepared according to Vauquelin's process, destroys vegetables nearly in the same manner that it does animals. Seeds soaked in this acid are equally killed by it, and lose their germinating power. Delicate plants are killed sooner than those which are stronger.—(Journal de Pharmacie, p. 174, April 1824.)

III. *Diurnal Variation of the Barometer.*

The Edinburgh Philosophical Journal, conducted by Dr. Brewster; the Journal of the Royal Institution of Great Britain, conducted by Professor Brande; the Bulletin Universal des Sciences et de l'Industin, published under the direction of Baron Ferassac, have announced that, according to a discovery made by Col. Wright, the mercury of the barometer, near the equator, rises and falls twice in 24 hours, with so much regularity, that this instrument may almost be employed as a measure of time.

We beg those readers of the Annales de Chimie, &c. who think that we communicate this discovery rather late, to remark that Goden, Bouguer, and Laundamire, made the same discovery nearly a century since; that since these academicians all travellers to the equinoxial regions have been engaged on the subject; that M. Humboldt, in 1807, published a very excellent work for the express purpose of making known the true hours of the *maxima* and *minima*, and the extent of the oscillation (vide Geograph. des Plantes); that Lamanon in the expedition under de Lapeyrouse, Horner in that of Krusenstern, &c. &c. undertook similar researches; that by means of the averages, Duc-Lachapelle, at Montauban, M. Ramond, at Clermont-Ferrand, the astronomers of the observatory at Paris, M. Marqué Victor, at Toulouse, &c. &c. have proved that this diurnal variation exists also in our climates: lastly, that we never omit, in our accounts of meteorological observations for the year, to give the amount of the daily falling of the barometer, from nine o'clock in the morning till three in the afternoon, and also of rising, which is evident between the last-mentioned time, and nine at night.—(Annales de Chimie et de Physique, t. xxv. p. 334.)

IV. *On the Cause of the Rotatory Motion of Camphor in Water.*

(To the Editor of the *Annals of Philosophy.*)

SIR, *Cambridge, May* 7, 1824.

The curious phenomenon of rotatory motion, which a particle of camphor presents when placed on the surface of water, I have frequently seen mentioned, but no where that I am aware of is there a cause assigned. In making a few experiments upon the subject, I was led to discover what I conceive to be the cause. It is a known law in hydrostatics that if a body floats on a fluid, the centre of gravity of the body and of the fluid displaced, must, when the body is at rest, be in the same vertical line; otherwise a rotatory motion is given to the body. In confirmation of this, if a perfectly smooth and square particle of camphor be placed on the surface of water, the camphor remains at rest. But if an uneven particle be made use of, then the centre of gravity of the body and of the water displaced are not in the same vertical line, and a rotatory motion is produced.

I remain your's, &c. E. A.

V. *On the Transmission of Electricity through other Fluids.*

DEAR SIR, *Charing Cross, May* 12, 1824.

Your number for April contains a letter from Mr. Woodward on the transmission of electricity through other fluids; allow me through the same channel to inform Mr. W. that the experiment of firing *loose* gunpowder by passing the charge of a Leyden phial through tubes filled with water, and also on the conducting power of alcohol, ether, and acids, were made by a Mr. Lewthwaite, in May 1821, and are published in the eleventh volume of the Institution Journal: it was from reading this letter that I became acquainted with the experiment, and have been pursuing it under a variety of forms; and with liquids of different conducting powers, which experiments, when well matured, shall be submitted to your consideration. Would suggest that Mr. W. should repeat the experiment with the water tube. I am disposed to think Mr. W. is in error, when he says the intensity (measured we are to suppose by a pith ball electrometer), indicated was from 10° to 15°. I have repeated the experiment several times, but always found a quart jar required an intensity of 65° to 70°; perhaps the pith ball attached to the electrometer was rather large; it would have been more satisfactory if the degree at which the jar spontaneously discharged itself had also been stated.

Yours, truly, T. J.

VI. *Volatility of Salts of Strychnia.*

M. Ferrari gives the following process for this purpose: solutions of salts of strychnia slightly acid when exposed to a heat of 212°, so as to be concentrated, then become volatile and the salt evaporates. This property has been remarked in the sulphate, nitrate, muriate, and acetate, and is believed to belong to all the salts. It has been remarked by M. Collaud and others, that the sulphate of quina is also volatile, and M. Ferrari, on repeating the experiments with the muriate and nitrate of quino, found it also to happen with them. The solutions on being heated in a tinned copper vessel, gave out vapours which, when breathed, were found to be highly bitter. The salts vary in the extent of this property, and it is also affected by the degree of acidity, and of concentration of the solution.—(Gior. de Fisica, vi. 460.)

VII. *Crystallization of the Sub-carbonate of Potash.*

M. Fabroni describes the following process for the crystallization of this salt. Make a solution of pearlash in water, and evaporate it until of specific gravity 1·57. Allow it to cool, when all extraneous salts will be deposited; separate the fluid and again concentrate it until of specific grav. above 1·6. The fluid will now be of a light green colour, and strong alkaline odour; place it in deep vessels, as glass jars for instance, and the sub-carbonate will soon crystallize in long rhomboidal white laminæ, situated vertically and parallel to each other; one extremity will touch the bottom of the vessel, and the other be attached to a saline crust on the surface of the liquid. When cold the mother liquor will be found of specific grav. 1·6, but if further consecrated and again cooled, more crystals will be obtained; and this may be continued until the whole has been crystallized.—(Ibid, 45.)

ARTICLE XIV.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Mr. Harris Nicolas has in the press, nearly ready for publication, a small work intended for the use of Antiquaries, Historians, and the Legal Profession, containing Tables that show exactly the year of our

Lord corresponding with the year of the reign of each Monarch; an Alphabetical and Chronological Calendar of Saints' Days, and other Festivals, on which ancient records are dated; Tables showing on what day of the month and week each moveable Feast occurred; an account of the Provincial Registries of Wills, with a list of the Parishes in each Diocese subject to Peculiar Jurisdictions; and a full description of the Contents of all the Works published by the Commission for the Preservation of the Public Records; with other useful matter.

Shortly will be published, in one volume, 8vo. An Excursion through the United States and Canada, in the Years 1822 and 1823; by an English Gentleman.

No. II. of the Zoological Journal, conducted by Messrs. Bell, Children, and Sowerby, will appear on the 15th of June.

JUST PUBLISHED.

Wade's Observations on Fever. 8vo. 4*s*.

Coddington's Optics. 8vo. 8*s*.

Boyle's Advice to Settlers in Tropical Climates. 18mo. 2*s*. 6*d*.

Plumbe on Diseases of the Skin, coloured Plates. 8vo. 14*s*.

Wollaston's Fasciculus Astronomicus, 1800. 4to. 1*l*.

Key on the Prostrate Gland. 4to. 12*s*.

Mementoes, Classical and Historical, of a Tour in France, Switzerland, and Italy. 2 vols. 8vo. 1*l*. 4*s*.

Prior's Life of Burke, with Portrait, &c. 8vo. 16*s*.

Hayward on Horticulture. 8vo. 12*s*.

Bell on the Spine and Thigh Bone. 16*s*.

Travers on the Eye. Third Edition. 1*l*. 5*s*.

Bostock's Elementary System of Physiology. 8vo. 15*s*.

Kitchener's Economy of the Eyes. 12mo. 7*s*.

ARTICLE XV.

NEW PATENTS.

J. H. Petelpiere, Chalton-street, Somers Town, engineer, for his engine or machine for making the following articles from one piece of leather without any seam or sewing whatever; that is to say, all kinds of shoes and slippers, gloves, caps, and hats, cartouch boxes, scabbards and sheaths for swords, bayonets, and knives.—March 20.

J. Rogers, Marlborough, Wilts, surveyor, for his improved instrument for determining or ascertaining the cubic contents of standing timber. —March 20.

J. Lingford, Nottingham, lace machine manufacturer, for certain improvements upon machines now in use for the purpose of making that kind of lace commonly known by the name of bobbin-net or Buckinghamshire lace net.—March 20.

J. Heathcoat, Tiverton, Devonshire, lace manufacturer, for certain improvements in machinery used in spinning cotton, wool, or silk.—March 20.

H. Berry, Abchurch-lane, London, merchant, for improvements on a machine for more readily producing light.—March 20.

J. J. Stainmare, Belmont distillery, Wandsworth-road, Vauxhall, for improvements in the process of distilling.—March 20.

C. Demeny, Fenchurch-street, merchant, for an apparatus for producing gas from oil and other oleaginous substances, of burning such gas for the purpose of affording light, and of replacing the gas consumed.—March 22.

N. Goodsell, Leigh-street, Burton Crescent, engineer, for a certain machine for breaking, scutching, and preparing flax and hemp for use, upon an improved method, and threshing out the seed thereof, and also for shelling clover and other seeds.—March 25.

E. Jordan, Norwich, engineer, for improvements in the construction of water-closets.—March 27.

J. Spencer, Belper, Derbyshire, nail manufacturer, for improvements in furnaces for the preparation of iron or steel, and for manufacturing nails and other articles from the said materials.—April 7.

J. Schofield, Rastrick, Halifax, Yorkshire, manufacturer, for certain improvements in the manufacture of cloth or fabric which he denominates British cashmere.—April 7.

T. Ryalls, Sheffield, warehouseman, for his apparatus for shaving, which he denominates "the useful and elegant facilitator."—April 8.

S. Hall, Basford, Nottinghamshire, cotton manufacturer, for his improved steam-engine.—April 8.

J. Tulloch, Savage Gardens, gentleman, for improvements in the machinery to be employed for sawing and grooving marble and other stone, or in producing grooves or mouldings thereon.—April 12.

H. P. Bevet, Devizes, Wiltshire, ironmonger, for his improvement in the construction of cranks, such as are used for bells and other purposes.—April 14.

W. By, Ivy Cottage, Ivory-place, Brighton, stationer and bookseller, for his method or apparatus for the preservation of books and covers.—April 14.

J. Gunby, New Kent Road, Surry, sword and gun manufacturer, for his improvement in the manufacturing of cases for knives, scissars, and other articles.—April 14.

D. Gordon, of Basinghall-street, for certain improvements in portable gas lamps.—April 14.

J. Beven, Manchester, dealer in cotton twist and weft, for his apparatus for dressing various kinds of cotton, flaxen, woollen, or silk manufactures.—April 14.

T. Gettien, Henry-street, Pentonville, for his improvements in the machinery of making metallic rollers, pipes, cylinders, and certain other articles.—April 15.

D. Tonge, Liverpool, ship-owner, for an improved method of reefing sails.—April 15.

A. Dallas, Northumberland-court, Holborn, engineer, for his machine to pick and dress stones of various descriptions, particularly granite stone.—April 27.

J. Turner, Birmingham, brass and iron founder, for his machine for crimping, plaiting, and goffering linen, muslins, frills, and other articles.—April 27.

G. Vaughan, Sheffield, Yorkshire, for his improvements on steam-engines, by which means power will be gained, and expense saved.—May 1.

ARTICLE XVI.

METEOROLOGICAL TABLE.

1824.	Wind.	Barometer.		Thermometer.		Evap.	Rain.
		Max.	Min.	Max.	Min.		
4th Mon.							
April 1	W	29·90	29·29	44	35	—	·45
2	N W	30·17	29·52	43	28	—	02
3	N	30·39	30·17	50	30	—	
4	N E	30·56	30·39	49	28	—	
5	N E	30·56	30·55	52	30	—	
6	N E	30·55	30·30	51	34	—	—
7	N W	30·38	30·29	45	33	—	·02
8	N E	30·38	30·21	54	38	—	
9	N E	30·21	29·53	50	38	—	13
10	N W	29·59	29·54	44	30	—	12
11	N W	29·57	29·55	45	30	—	01
12	N W	29·80	29·57	52	30	—	—
13	N W	30·00	29·80	55	30	·85	—
14	N W	30·00	29·99	52	25	—	—
15	E	29·99	29·50	56	38	—	·06
16	E	29·60	29·46	44	40	—	·80
17	N E	30·18	29·60	48	34	—	20
18	N E	30·39	30·18	58	28	—	
19	S E	30·43	30·36	59	33	—	
20	E	30·43	30·23	65	34	—	
21	E	30·23	30·02	62	51	—	04
22	S W	30·07	29·69	65	48	—	09
23	Var.	30·09	29·65	64	45	·95	08
24	N W	30·27	30·09	60	39	—	
25	S W	30·24	29·85	64	51	—	
26	S E	29·92	29·85	62	48	—	—
27	W	30·04	29·92	63	52	—	
28	S W	30·04	29·92	60	50	—	02
29	S W	29·92	29·79	73	51	—	
30	S W	30·05	29·79	66	50	·96	01
		30·56	29·29	73	25	2·76	2·05

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Fourth Month.—1. Fine till five, p. m.: wind cold; night rainy. 2. Some rain this morning: cloudy: windy. 3—6. Fine. 7. Cloudy. 8, 9. Fine. 10. Rainy morning: stormy day: showers of hail and rain: gusty. 11. The ground covered with snow this morning; snow showers. 12. Cloudy. 13. Fine: two or three trifling showers. 14. Ditto. 15. Fine: very high wind during the night: a large solar halo, slightly tinged with prismatic colours, which lasted about half an hour just before sunset. 16. Very rainy morning, with high wind. 17. Rainy morning: wet till four, p. m. 18. Very fine morning. 19—22. Fine. 23. Showery morning: overcast: windy. 24, 25. Fine. 26. Very much overcast this morning: showers, p. m. 27. Overcast. 28. Showers. 29, 30. Fine.

RESULTS.

Winds: N, 1; E, 4; SW, 5; SE, 2; W, 2; NW, 8; NE, 7; Var. 1.

Barometer: Mean height

For the month	30·009 inches.
For the lunar period, ending the 20th	30·025
For 13 days, ending the 10th (moon north)	30·088
For 14 days, ending the 24th (moon south)	29·935

Thermometer: Mean height

For the month	46·85°
For the lunar period	40·216
For 30 days, the sun in Aries	39·966

Evaporation	2·76 in.
Rain	2·05

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